ESSAYS

PHYSICAL AND CHEMICAL,

BY

M. LAVOISIER,

Member of the ROYAL ACADEMY of SCIENCES at PARIS, &c.

VOLUME THE FIRST.

TRANSLATED FROM THE FRENCH,

WITH NOTES,

AND AN

APPENDIX,

By THOMAS HENRY, F.R.S.

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J. Mason Warren M.D.

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TO

SIR JOHN PRINGLE,

BARONET,

PRESIDENT of the ROYAL SOCIETY,

AND

PHYSICIAN IN ORDINARY TO

HIS MAJESTY.

SIR,

THE subject of M. LAVOISIER'S Philosophical and Chemical Essays, has already been honoured with your attention; and the praise which you have so justly bestowed on our very ingenious countryman Dr. PRIESTLEY, in your elegant and learned oration, on delivering to him the honourable reward of his refearches into the nature and properties of different kinds of air, evinces the degree of importance in which you efteem fuch inquiries. The recent difcoveries, which have refulted from them, fully juftify the decifion of the ROYAL SOCIETY; and it muft be pleafing to every individual of it to obferve, in perufing the hiftorical part of M. LAVOISIER's Treatife, the very large fhare which their brother-members have fuftained in this interefting inveftigation.

To you therefore, Sir, as Presi-dent of the firft Philofophical So-ciety in Europe, as a moft compe-tent judge of the merit of the ori-ginal work, I beg leave to dedicate this Tranflation, and requeft you to accept it as a teftimony of the gratitude, efteem and refpect with which I am,

SIR,

Your moft devoted

and obliged

humble Servant,

MANCHESTER,
Feb. 1ft. 1776.

THOMAS HENRY.

THE TRANSLATOR's

PREFACE.

IT has been obſerved by one of the greateſt Philoſophers of the preſent age, that "if thoſe who unhappily ſpent their time and ſubſtance in ſearch after an imaginary production that was to reduce all things to gold, had, inſtead of that fruitleſs purſuit, beſtowed their labour in ſearching after that much neglected volatile *Hermes*, who has ſo often eſcaped through their burſt receivers in the diſguiſe of a ſubtle ſpirit, a mere flatulent exploſive matter; they would then, inſtead of reaping vanity, have found their reſearches rewarded with very confiderable difcoveries." *

It will appear that this obfervation has been ftrongly verified. Since men have ceafed to pay attention to the arrogant pretenfions and idle dreams of the old Alchemifts, and have directed their inquiries on phyfical principles, a very rapid progrefs has been made in the improvement of Chemiftry. Myftery and empyricifm have given place to fyftematical perfpicuity; men of the firft character in Philofophy, and of the higheft rank and opulence, have become cultivators of the fcience, and Chemiftry, inftead of confining her purfuits to the tranfmutation of metals or the difcovery of panaceas, has now taken a more liberal and enlarged field of action, and has greatly contributed, and, it is hoped, from the progrefs which fhe every day makes, will ftill more extenfively contribute

* Hales's Statics, Vol. I. p. 316.

tribute to the improvement of the other arts and ſciences. By her aſſiſtance Philoſophers have been enabled to make greater diſcoveries in a few years, than they were before capable of effecting in an age. Inſtead of building on the ſandy foundation of hypotheſis, they now eſtabliſh more durable ſyſtems ſupported by experiment and rational induction. Theſe are the trials to which every new opinion is to be ſubmitted; and, however plauſible its appearance or reſpectable its inventor, no theory can be admitted which will not ſtand the teſt of this examination.

The advantages ariſing from the aid which Chemiſtry affords to the other branches of Phyſics, have been in nothing more conſpicuous than in the very important diſcoveries which have been lately made relative to the nature and conſtitution of Air. By the aſſiduous application and unremitting attention of ſeveral eminent Philoſophers;

phers, and particularly among our own countrymen of Meſſieurs Hales, Black, Macbride, Cavendiſh and Prieſtley, a number of experiments have been made, by which the analyſis of this fluid has been purſued much farther than could poſſibly have been expected; and hence many new and curious diſcoveries have reſulted, which were wholly unforeſeen even by the ingenious experimentaliſts themſelves.

Besides the moſt convincing demonſtration of the exiſtence of air, in a fixed ſtate, in ſeveral of the hardeſt and moſt ſolid bodies, capable notwithſtanding of recovering its elaſticity whenever it is let looſe from its baſis by the action of fire, or by fermentation or effervеſcence; beſides the proof that it is contained in great abundance in calcareous earths and alkaline ſalts, and, that as on its preſence depend ſome of their diſtinguiſhing properties, ſo they acquire new ones by being

deprived

deprived of it; that this air has various degrees of affinity with different bodies, and is capable of being transferred from one ſubſtance to another; it has alſo been diſcovered that fixed air, when reſtored to a ſtate of expanſibility, is different from common air, fatal to animals who breathe it, yet ſtrongly antiſeptic, not only reſiſting the putrefaction of animal ſubſtances, but even reſtoring them to ſweetneſs and firmneſs when actually putrid, and that, contrary to common air, it may be abſorbed in conſiderable quantity by water.

These laſt properties of fixed air, ſuggeſted the idea of its utility as a medicine in putrid diſeaſes; the trials that have been made have fully juſtified the expectations that had been formed of its efficacy. We are arrived at the power of imitating the acidulous and chalybeate waters in great perfection, and fixed air has been adminiſtered

niſtered in various forms, and with conſiderable ſucceſs, in ſeveral diſeaſes.

However firmly the doctrine of fixed air might appear to be eſtabliſhed in Great Britain, ſome formidable opponents to it aroſe in Germany. Theſe Philoſophers, inſtead of fixed air, ſubſtituted another agent which they denominated *Acidum pingue*, with this difference, that as the cauſticity of alkalis and quick lime, and the ſolubility of the latter in water, depend, according to Dr. Black's ſyſtem, on their being *deprived* of fixed air, the German theory repreſents theſe properties as depending on thoſe bodies *poſſeſſing* the *acidum pingue*, which neutralizes them, and thereby gives ſolubility to the one, and cauſticity to both.

The deciſion of this controverſy, of ſuch importance to ſcience, has been undertaken by M. Lavoiſier of Paris, a Gentleman

tleman of diſtinguiſhed rank, and an Intendant of the Finances, who, amidſt the various public avocations to which the nature of his office ſubjects him, purſues a number of philoſophical and chemical reſearches with almoſt unequalled abilities and perſeverance. The hiſtory which he has given us of the gradual diſcoveries which have been made, relative to the ſubject of elaſtic vapours, is ſo entertaining and inſtructive, and his experiments ſo well conducted, his inferences, in general, ſo juſtly, ſo judiciouſly deduced, that I thought I could not render a greater ſervice to the public, than by tranſlating M. Lavoiſier's Treatiſe into Engliſh, being ſenſible that the reading of foreign books of ſcience in their original language, is, from various cauſes, confined to a very narrow circle.

Many of my readers will perhaps be convinced that this remark is not ill founded, ed, when they ſee how much has been done abroad on the ſubject of fixed air. I am apt to think that the modern foreign authors, of whoſe works M. Lavoiſier has given a detail, are far from being generally known in this iſland; yet they ſeem to be diſtinguiſhed by an ingenuity, and a degree of phyſical knowledge well deſerving of our candid attention.

M. LAVOISIER, in his account of the diſcoveries of our great Engliſh Philoſopher, has, as I imagine from an inſufficient acquaintance with our language, ſometimes miſrepreſented that Author's meaning. Where this was the caſe, I have either altered the text by reſtoring Dr. Prieſtley's own words, or, where that could not be ſo conveniently done, have pointed out and corrected the miſtakes by notes. I have likewiſe, as M. Lavoiſier's hiſtory only recounts Dr. Prieſtley's experiments as publiſhed in the Philoſophical Tranſac-

tions,

tions, with a view to make the hiſtorical part more complete, added a ſhort account of his more recent diſcoveries contained in the firſt volume of his Experiments and Obſervations on different kinds of Air. In order to diſtinguiſh theſe notes from thoſe of M. Lavoiſier, I have marked them with the initial letters of my name.

I SHOULD have been apprehenſive, from my very ſhort acquaintance with the French language, of publiſhing this Tranſlation, had not my friend Mr. Aikin of Warrington kindly undertaken to reviſe the ſheets. His perfect knowledge of the language and of the ſubject, have certainly contributed to make it more free from error, than I fear it would otherwiſe have been.

A MEMOIR of M. Lavoiſier's, read before the Royal Academy of Sciences, *on the nature of the principle which combines with metals during their calcination and*

and increaſes their weight, has been, lately, put into my hands, and I thought it proper to add this by way of Appendix; and the publication of Dr. Prieſtley's ſecond volume on the ſubject of Air, has enabled me, with that Gentleman's approbation, not only to give his ſentiments on the nature of that principle, but alſo his ideas of the conſtitution of common air. But ſo much intereſting information, ſuch important diſcoveries abound in that work, that nothing but a peruſal of the whole can ſatisfy any perſon poſſeſſed of the leaſt philoſophical taſte.

ADVERTISEMENT

PREFIXED TO THE

FRENCH EDITION.

DURING more than ten years that I have applied myſelf to Phyſics and Chemiſtry, and have devoted, to theſe two ſciences, the time of which my other occupations have permitted me to diſpoſe, my materials have ſo greatly accumulated, that I cannot poſſibly expect they will find a place in the Collection of Memoirs of the Royal Academy of Sciences. Beſides, moſt of the objects which have employed my attention have required too great a number of experiments, and too great an extent of diſcuſſion, to leave it poſſible for me to confine them to the bounds prescribed to our Memoirs, and I therefore thought myself indispensibly obliged to form them into distinct Treatises.

The diversity of subjects which I have to offer to the public, the uncertainty also, under which I remain, to determine in what order I shall publish my Memoirs, have imposed on me the necessity of chusing a general title applicable to the whole, and that of Physical and Chemical Essays appeared to me more proper than any other to answer my intention. This title will apprise the reader of the indulgence for which I have occasion; it will give me the liberty of presenting him with detached observations; and lastly, it will even render excusable the very disorder which may appear in the arrangement of the materials.

We easily become interested for the subject on which we are employed, and the

last

laſt labour in which we have been engaged is commonly the favourite object. This weakneſs, from which it would be difficult, and perhaps dangerous to guard ourſelves, has doubtleſs induced me to publiſh firſt what I have collected concerning the exiſtence of an elaſtic fixable fluid in ſome ſubſtances, and its diſengagement from them, although this work is the laſt I have written; beſides the kind of intereſt, which men of ſcience ſeem to take in this ſubject, and the reſearches which multiply on every ſide, would certainly have been a ſufficient motive to determine me, and it is unneceſſary therefore to ſearch for any other.

I DESIGNED to have inſerted, in this volume, ſome much more elaborate details on the precipitation of metals diſſolved in acids, and the conſiderable augmentation in weight which they acquire in that operation; but the neceſſity of firſt inveſtigating the nature of the acids themſelves, of knowing the principles of which they are compoſed, and the caſes in which their decompoſition is effected, &c. prevented me; and I have been convinced that many things were previouſly to be done. From theſe and other ſimilar motives, I have alſo deferred the publication of my experiments on fermentation in general, and on the acid fermentation in particular.

This firſt volume will, I hope, be followed by ſeveral others, and in theſe I ſhall ſucceſſively enter on a train of experiments which are already numerous, and which I ſtill intend to increaſe; 1ſt, on the exiſtence of the ſame elaſtic fluid in a great number of bodies in nature, in which it has not been hitherto ſuſpected; 2dly, on the total decompoſition of the three mineral acids; 3dly, on the ebullition of fluids in the vacuum of an air pump; 4thly, on a method of determining the quantity of ſaline matter contained in mineral

neral waters, from the knowledge of their ſpecific gravity; 5thly, on the application of the uſe, either of pure ſpirit of wine, or of ſpirit of wine mixed with water, in certain proportions, to the analyſis of the very complicated mineral waters; 6thly, on the cauſe of the cold which is obſerved in the evaporation of fluids; 7thly, on different points of optics, on which I have had occaſion to be employed in a Memoir relative to the lighting of the ſtreets of Paris; a work which the Academy have been pleaſed to reward, in their public meeting at Eaſter 1766, with a gold medal, and in which I have ſince had occaſion to make conſiderable alterations and additions; 8thly, on the height of the principal mountains in the environs of Paris, compared with the level of the river Seine, meaſured both by the aid of a good quadrant belonging to M. le Chevalier de Borda, and alſo of an excellent level with a bubble of air and a glaſs, conſtructed by

M. de Chezy, and the property of M. Perronet: Laftly, I fhall add a numerous train of obfervations on the barometer, made in different provinces of France; I fhall give a fketch of the inner part of the earth in thefe provinces to a pretty confiderable depth, the order which is obferved in the ftrata, the conftant level at which certain fubftances and certain fhells are found, and the remarkable inclination which fome ftrata always have in the fame direction.

These different works are moftly far advanced, feveral of them have even been long fince figned by M. de Fouchy, perpetual Secretary to the Academy, I therefore expect that it will foon be in my power to fubmit them to the judgment of the public.

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ERRATA.

Page 47, line 14, *after* matter *place a* comma.
50, 12, *for* primariam *read* primarium.
117, 6, *in the note, for* it *read* them.
128, *note, dele* inverted commas.
159, line *last, in the note, for* contain *read* contains.
183, 4, *in the note, after* water *read* with fixed air.
184, 21, *ditto, for* nitrous *read* noxious.
— 5, *upwards ditto, dele* inverted commas after words honour and remarks.
185, 10, *ditto, after* years *add* after.
197, 11 and 19, *for* del *read* delle.
200, 3, *for* be *read* is.
— 3, *last note, dele* other.
213, 2 *from the bottom, after* burns *place a* semicolon.
221, 10, *after* salts, *instead of a* period *a* comma.
227. 12, *dele* and.
235, 7, *for* a *read* but.
260, 12, *for* contains *read* contain.
326, 2 *from the bottom, for* as *read* a.
352, line *last, for* come *read* comes.

THE

FIRST PART.

A SUMMARY HISTORICAL ACCOUNT OF THE ELASTIC VAPOURS WHICH ARE SEPARATED FROM BODIES, DURING EITHER THEIR COMBUSTION, FERMENTATION OR EFFERVESCENCE.

A SUMMARY

HISTORICAL ACCOUNT

Of the *ELASTIC VAPOURS* which are separated from bodies during either their combustion, fermentation or effervescence.

THE

INTRODUCTION.

A GREAT number of foreign Philosophers and Chemifts are at this time employed in refearches concerning the fixation of Air in bodies, and the elaftic vapours which are feparated in the combination as well as in the decompofition and refolution of their principles. Various Memoirs, Thefes and Differtations have appeared on this fubject in England, Germany, and Holland.

 The

The French Chemifts alone feem not to take any part in thefe important inquiries; and while the difcoveries of other nations increafe every year, our modern publications, the moft complete, in many refpects, of any that have been written in Chemiftry, are almoft totally filent upon this fubject.

These confiderations induced me to think it neceffary to give the public a fhort account of every thing, which has hitherto been done, relative to the combination of Air with bodies, and to give an accurate defcription of the difcoveries which have been made in this fubject. This I propofe to do in the firft part of this Treatife. I have endeavoured to perform it with the utmoft impartiality, and I have confined myfelf, as much as poffible, to the fimple character of an Hiftorian.

In the fecond part, an account will be given of my own experiments. Thofe which are related in the firft Chapter, are intended to fix the opinion of Chemifts in regard to the different fyftems of Dr. Black and Mr. Meyer. I believe I have

I have arrived to as great a degree of certainty, in this refpect, as can be hoped for in Phyfics. The following Chapters treat of the union of the elaftic fluid with the metallic calces, of the burning of Phofphorus, of the formation of its acid, of the nature of the elaftic fluid difcharged in the folutions of metals, &c. &c.

It muft be acknowledged that this laft part of my work is not fo complete as I could wifh, and that it is not even without fome degree of regret that I publifh it; But as it is eafy to lofe one's way in a road but little travelled, I reflected how important it might be for me to benefit myfelf by the remarks of the learned, though it might expofe me at the fame time to their criticifms. It is principally with this view that the latter part of this work is publifhed in this ftate of imperfection; and I am already aware that I have occafion for all the indulgence of my Reader.

CHAPTER I.

Of the ELASTIC FLUID described, to the time of PARACELSUS, under the name of SPIRITUS SILVESTRIS, and under the name of GAS by VAN HELMONT.

THE different Authors, previous to Paracelſus, who have ſpoken of the elaſtic matter which is ſeparated from bodies either by means of Fire, Fermentation or Efferveſcence, do not ſeem to have formed very clear ideas of its nature and properties. They have given it the name of Spiritus Silveſtris.

Paracelsus, and ſome Authors cotemporary with him, imagined this matter to be the ſame as the air which we breathe; but it does not appear that this opinion was ſupported among them by any argument, much leſs by Experiments. Van Helmont the diſciple of Paracelſus, and

and frequently differing in opinion from him, appears to be the firft who undertook to make the following inquiries into the nature of this fubftance. He gave it the name of Gas *, Gas Silveftre †, and he defined it a fpirit, an incoercible vapour, which could neither be collected in veffels nor be reduced under a vifible form. He obferves that fome bodies refolve themfelves, almoft entirely, into this fubftance; "Not, adds he, that it had actually been contained under that form in the bodies from which it was feparated; otherwife nothing could have retained it, and all its parts would have been diffipated. But it was contained under a concrete form, as if fixed or coagulated". This fubftance, according to Van Helmont's experiments, is feparated from every fermenting body; from wine, from meath, from

* GAS is derived from the Dutch word Ghoaft, which fignifies Spirit. The Englifh exprefs the fame idea by the word Ghoft, and the Germans by the word Geift, which is pronounced Gaiftre. Thefe words have too much affinity with that of Gas, to leave any doubt of their derivation.

† Complexionum atque mixtionum Elementalium Figmentum. No. 13, 14, & feq.

from verjuice and from bread: it may be disengaged from ſal ammoniac, in the way of combination, and from vegetables, by the action of fire*. This is the matter which eſcapes from gunpowder when inflamed; and which iſſues forth from charcoal when burning. The Author, on this occaſion, aſſerts that ſixty two pounds of charcoal, contain ſixty one pounds of Gas, and only one part of earth.

It is, alſo, to the effluvium of Gas, that Van Helmont attributes the fatal effects of the Grotto delle Cani in the kingdom of Naples †, the ſuffocation of the workmen in the mines, the accidents occaſioned by the vapour of charcoal, and that deliterious atmoſphere which is breathed in cellars where ſpirituous liquors are in fermentation.

The great quantity of Gas which flies off from earths or from metallic ſubſtances when in effervеſcence with acids, did not eſcape the notice

* Tractatus de Flatibus, No. 67.

† Complexionum atque Mixtionum Elementalium Figmentum, No. 43.

tice of Van Helmont *; the quantity contained in tartar is ſo great, that it breaks and burſts into ſhivers the veſſels in which it is diſtilled, if a free egreſs be not given to it.

Van Helmont in his Treatiſe *de Flatibus*, applies this theory to the explanation of ſome phenomena of the animal œconomy. He aſſerts, No. 36, that it is to the corruption of the aliment, and to the Gas diſengaged from it, that we ſhould attribute wind, the diſcharges of it from the bowels, &c. and he has given us, on that occaſion, a very well formed theory of the phenomena of digeſtion. He alſo accounts, from the ſeparation of this Gas, for the ſwelling of dead bodies which have remained ſome time in the water, and for the tumours which ariſe on ſome parts of the body in certain diſeaſes. We are aſtoniſhed, in reading this Treatiſe, to find an infinite number of facts, which we are accuſtomed to conſider as more modern, and we cannot forbear to acknowledge, that Van Helmont has related, at that period, almoſt every thing, which we are now better acquainted with, on this ſubject.

* Tractatus de Flatibus, No. 67 & 68.

It is in the ſame Treatiſe * that Van Helmont examines whether this Gas or Spiritus Silveſtris of the Ancients be not, according to Paracelſus's opinion, the ſame air which we breathe reduced to its elementary parts and combined with bodies. Although the arguments and experiments, by which he ſupports his opinion, be not very deciſive, he believes, however, that it may be determined † that the Gas is different from the air which we breathe; that it has a greater affinity with water; that it probably might conſiſt of water reduced to vapours. Next moment ‡ he thinks that this air might perhaps reſult from a combination of a very ſubtle acid with the volatile alkali.

The paſſages of Van Helmont's works which I have quoted, are not the only ones in which he ſpeaks of the Gas: he has taken notice of it in many others, and particularly in his Treatiſe *de Lithiaſi*, Cap. 4. No. 7. and in his *Tumulus peſtis*. He alſo aſcribes the propagation of epidemic diſeaſes to the vapours with which Gas is infected.

CHAP-

* De Flatibus, No. 19.

† Idem.

‡ Idem, 67 & 68.

CHAPTER II.

Of Mr. BOYLE's ARTIFICIAL AIR.

WHAT Van Helmont called *Gas*, Boyle denominated *artificial Air*. Furniſhed with the new inſtruments with which he has enriched Natural Philoſophy, he repeated all Van Helmont's experiments in Vacuo, in condenſed Air, and in open Air. The greater part of his Experiments may be found in a work entitled, *Continuatio novorum Experimentorum phyſico mechanicorum de gravitate et elatere aeris.* Some others are diſperſed in many parts of his works.

Boyle was ſenſible, as well as Van Helmont, that moſt vegetables, when diluted with a certain proportion of water, and placed in a ſituation proper for fermentation, diſcharge a conſiderable quantity of Air; that this Air diſcharges itſelf with more facility in the exhauſted receiver of an Air pump, than in compreſſed

Air;

Air; that every thing which impedes the progreſs of fermentation ſuſpends alſo the ſeparation of the air, and that ſpirit of wine poſſeſſes this property in a particular and eminent degree.

These experiments, repeated in air much more condenſed than that of the atmoſphere, afforded nearly the ſame reſults. He then tried the effect of placing the fermenting matter in an atmoſphere of artificial air, and he found that in ſome circumſtances it accelerated, and in others retarded the fermentation. But one eſſential difference, between this air and that of the atmoſphere, before obſerved by Van Helmont and recogniſed by Boyle, is that the latter is neceſſary to the exiſtence of many animals, whereas the other, when breathed by them, proves inſtantly deſtructive. Mr. Boyle's experiments on this ſubject demonſtrate that artificial air is not always the ſame, from whatever vegetable ſubſtance it may be ſeparated; and that the air which is produced by the exploſion of gunpowder preſents phenomena peculiar to itſelf.

It is eaſy to ſee that almoſt all the diſcoveries of this kind, which we have uſually attributed to Mr. Boyle, really belong to Van Helmont, and that the latter had even carried his theory much farther. But one obſervation which is particularly Boyle's, does not ſeem to have been even ſuſpected by Van Helmont, viz. That there are bodies, ſuch as ſulphur, amber, camphor, &c. which diminiſh the volume of air, in which they burn.

CHAPTER III.

DR. HALES's EXPERIMENTS ON THE QUANTITY OF ELASTIC FLUID SEPARATED DURING THE COMBINATIONS OR DECOMPOSITIONS OF BODIES.

THE united experiments of Van Helmont and of Boyle had made it evident that a great quantity of elaſtic fluid analogous to air was ſeparated from bodies in many operations; that alſo in ſome other operations a portion of atmoſpheric air was abſorbed, or at leaſt deprived of of its elaſticity; but nobody had conceived any idea of the quantities either produced or abſorbed. Dr. Hales was the firſt perſon who has ſeen the object in this point of view; he invented various methods, equally ſimple and commodious, to meaſure with exactneſs the bulk of air. I ſhall not at preſent attempt to deſcribe the different inſtruments which he made uſe of. Particular attention will be paid to this object in the ſequel; I ſhall then deſcribe the alterations which have been made in them by ſome Philoſophers, and thoſe which I think they are capable of receiving.

The great number of experiments made by Doctor Hales, which may be found in Chap. 6. of his Vegetable Statics, comprehends almoſt every ſubſtance in nature. He has examined into the effects of combuſtion, of fermentation and of combinations, &c. As theſe experiments are, even at this time, the moſt complete of their kind, it may be proper to give, in this place, an abſtract of them. The form of a Table appeared to me to be the moſt clear, the moſt convenient, and the leaſt voluminous.

EXPE-

EXPERIMENTS

BY DISTILLATION.

The Names of the Materials uſed in the Experiments.	The number of cubic inches of air produced by diſtillation.
ON VEGETABLES.	
One cubic inch or 270 grains of oak timber,	256
One cubic inch or 398 grains of peaſe,	396
142 grains of dry tobacco, — —	153
One cubic inch of oil of aniſeeds, —	22
One cubic inch of oil of olives, — —	80
One cubic inch of tartar, — —	504
One cubic inch or 270 grains of amber,	270
OF ANIMAL SUBSTANCES.	
One cubic inch of hog's blood diſtilled to dryneſs, — — —	33
Rather leſs than a cubic inch of tallow,	18
One cubic inch or 482 grains of hartſhorn,	234
One cubic inch or 532 grains of oyſter ſhells,	324
One cubic inch of honey, — — —	144
One cubic inch or 253 grains of yellow wax,	54
Three-quarters of a cubic inch of human calculus weighing 230 grains, —	516

OF

Names of Materials, &c.	Number of cubic inches of Air, &c.
OF MINERALS.	
One cubic inch or 316 grains of pit-coal,	360 *
One cubic inch of fresh untried earth, —	43
One cubic inch of antimony, — —	28
Half an inch of sea salt and the same quantity of calcined bones, — —	64
Half a cubic inch or 211 grains of nitre, with the calx of calcined bones, —	90

EXPERIMENTS

ON FERMENTATION.

Forty-two inches of small beer in seven days,	639
Twenty-six inches of bruised apples in thirteen days, — — —	968

EXPE-

* About 102 grains of Air, according to Dr. Hales, or a third of the whole weight.

EXPERIMENTS

ON DISSOLUTIONS AND COMBINATIONS.

The Names of the Materials ufed in the Experiments.	Cubic inches of Air produced.	Cubic inches of Air abforbed.
Half a cubic inch of fal ammoniac with one cubic inch of oil of vitriol, the firft day, —	5 to 6	
The next day it had abforbed 15.		
Six cubic inches of oyfter fhells and as much vinegar in fome hours, — — —	29	
In nine days 21 inches were deftroyed, and the other eight difappeared on pouring warm water into the veffel *.		

Two

* "The ninth day, fays Dr. Hales, I poured warm water into the "veffel, and the following day, when all was cool, I found that it had "reforbed the remaining eight cubic inches. Hence we fee that warmth "will fometimes promote a reforbing as well as a generating ftate, "viz. by raifing the reforbing fumes as will appear hereafter."

Dr. Hales here afcribes to the warmth what was really owing to the water fimply as fuch, which from its affinity with fixed air imbibed it with rapidity. And indeed from the Doctor's own relation it does not appear that the reforption took place while the mixture continued warm. T. H.

Names of Materials, &c.	Inches of Air produced.	Inches of Air abſorbed.
Two cubic inches of aqua regia poured on a gold ring flattened,	4	
Two cubic inches of aqua regia with a quarter of an inch of antimony, in three or four hours,	38	
After ſome hours 14 inches were reſorbed.		
A cubic inch of aqua fortis poured on a quarter of an inch of antimony, at different intervals,	130	
A cubic inch of aqua-fortis, with $\frac{1}{4}$ of an inch of iron filings,	43	
Quarter of an inch of iron filings, and one cubic inch of powdered ſulphur, — — — —	— —	19
A cubic inch of aqua-fortis on an equal quantity of powdered marcaſite, or Walton pyrites,	— —	85
A cubic inch of aqua-fortis, with the ſame quantity of pit-coal, abſorbed in three days 18 inches of air; and in three days more remitted and generated 12 cubic inches, — — —	— —	18
Two cubic inches of quick-lime, with four of vinegar, — —	— —	22

Two

Names of Materials, &c.	Inches of Air produced.	Inches of Air abſorbed.
Two cubic inches of lime and as much ſal ammoniac, — —		115
By the burning of linen-rags, dipped in melted brimſtone, in a large veſſel, — — —		198
In a ſmaller veſſel, — — —		150
Two grains of Kunkel's phoſphorus, — — —		28
After inflammation it had loſt only half a grain; ſome time after its weight was augmented one grain.		
A piece of brown paper ſoaked in a ſtrong ſolution of nitre, and ſet on fire under a receiver, by means of a burning lens, produced — — —	80	
In a few hours this quantity of air was diminiſhed.		

EXPERIMENTS

ON BURNING BODIES,

AND ON THE

RESPIRATION OF ANIMALS.

The Names of the Materials ufed in the Experiments.	Cubic inches of Air produced.	Cubic inches of Air abforbed.
A lighted candle, of $\frac{3}{5}$ of an Englifh inch in diameter,		78
A rat confined in a receiver,		78
73 cubic inches of air, breathed by a man till he was nearly fuffocated, was reduced to 20 inches.		

THESE are not the only experiments contained in the fixth chapter of Dr. Hales's Vegetable Statics; we meet with feveral others which are not capable of being reduced to the form of a table. The Author has every where joined to them views entirely new and excellent reflections. Too much cannot be faid to induce the reader to perufe

perufe that Author's own work. He will find a moft inexhauftible fund of meditation. I fhall now proceed and endeavour to give an abftract of the fixth chapter of his book, though it be not very capable of abridgement.

It is in this work, that we perceive the firft traces of the exiftence of air in thofe waters which are, even now, improperly called acidulous. Dr. Hales has not only remarked that thefe waters contained double the quantity of air to common water, but he alfo fufpected that they owed their remarkable fparkling and brifknefs to this air.

Though Dr. Hales fufpected that the acids in general, and particularly fpirit of nitre, contained air, yet the diftillation of aqua fortis gave a contrary product. Inftead of the augmentation which he expected, he obferved a remarkable diminution of the volume of air. The confequence which he drew, from this obfervation, was that acid vapours abforb air; and from hence he concluded, that what we obtain by the combination of acids with alkaline fubftances may not wholly appertain to the latter, but that the acid may alſo afford a part of it, and that it is very probable that it is this ſubſtance which produces the air which is ſeparated in ſolutions of metals by acids.

It is to the great quantity of air which is let looſe by the detonation of nitre, that Dr. Hales attributes the effects of gunpowder; to which he, notwithſtanding, thinks ſhould be added the expanſion of the watery particles which are reduced into vapour. If tartar, which, as well as nitre, contains a very large quantity of air, does not explode in the ſame manner, Dr. Hales accounts for this difference, by ſuppoſing that the air is more intimately united to it, and requires a greater degree of heat to detach it; and it is from this great quantity of air and its cloſe adherence to the tartar that he deduces an explanation of the effects of the pulvis fulminans.

Dr. Hales has attempted to determine the ſpecific gravity of Air, which had been ſeparated from tartar, by diſtillation; but he did not find that it differed at all, in that reſpect, from atmoſpheric air; the event was the ſame, whether he he employed air *recently* extracted from tartar, or air which had been ſeparated more than ten days before.

It did not eſcape the knowledge of Dr. Hales that the quantity of air abſorbed either by the burning of ſulphur or of candles, or by the reſpiration of animals, preſented different appearances, accordingly as he employed receivers of a larger or ſmaller capacity. He obſerved, in this reſpect, that the quantity of air abſorbed is generally greater in large than in ſmall veſſels; that, however, it is more conſiderable in ſmall than in large veſſels, *in proportion to their different capacities*. He alſo remarked that this abſorption of air is limited; and that it cannot proceed beyond a certain point.

Dr. Hales, in the courſe of his experiments, has obſerved the ſingular, alternate production and abſorption of air, of which he does not ſeem to have underſtood the true cauſe: the detonation of nitre, for example, furniſhed him with a great quantity of air; but that air diminiſhed daily in its elaſticity and bulk: he has obſerved the ſame thing in regard to a great number of different factitious kinds of air. This phenomenon depended on the water which the Doctor always uſed in his experiments: it will be ſhewn hereafter, that moſt of theſe vapours, and particularly thoſe which we are accuſtomed to denominate Fixed Air, have a great affinity with water, which is capable of abſorbing more than its own bulk of this air. Hence it was, that Dr. Hales did not always draw juſt concluſions from his experiments, and this was the ſource of errors, of which he was by no means appriſed, and which will make it neceſſary to repeat his trials, ſome time, with particular precautions.

It is to this tendency of fixed air, to unite itſelf with water, that we may attribute the phenomenon, obſerved by Dr. Hales, in the burning of candles. He remarked that the abſorption of air continued, not only during their burning, but even ſeveral days after. It will be ſeen, hereafter, in the Chapter which treats of the Experiments made by Dr. Prieſtley, that the air in which candles have burned, is, in a great meaſure, in the ſtate of fixed air; that it is conſequently

ſequently ſuſceptible of combination with water; and that it is in proportion to this combination that the volume of air continues to diminiſh. It was, alſo, owing to this cauſe that the different kinds of air, which he obtained, did not appear to be ſuſceptible of farther reduction, after they had paſſed through water. In fact, all the fixable part had combined itſelf with the water.

The air in which ſulphur has burned is incapable of recovering its elaſticity; it remains in the ſame ſtate, however long a time it may be kept.

Dr. Hales, perſuaded that the air ſeparated from bodies, as well as that which had been burned, or been reſpired by animals, was not different from that of the atmoſphere, and that it produced ſuch particular effects, only on account of its being infected and rendered noxious by vapours which were foreign to its nature, attempted to filter it through flannel which had been ſteeped in a ſolution of ſalt of tartar; and, by this means, he perfectly reſtored it. The air, when paſſed through the filter, was found fit for refpiration. A candle, likewife, placed under a receiver lined with a flannel which had been dipped in a folution of falt of tartar, burned a longer time than it could have done under a receiver which was not lined, although the flannel confiderably diminifhed its capacity. We fhall, in future, fee what was the effect of the falt of tartar in this experiment, and in what manner it rendered the air falubrious; but one interefting fact fhould be, here, remarked, viz. that the flannel diaphragms, through which the air had been filtered, were fenfibly increafed in their weight.

It is to Dr. Hales alfo that we are obliged for the information, that a great number of fubftances, fuch as peafe, wax, oyfter fhells, amber, &c. furnifh, by diftillation, air fufceptible of inflammation, and which even retains that quality, after it has been wafhed in water.

All the Philofophers of his time believed, that fire became fixed, and combined itfelf with metals, and that to this addition they owed their reduction to the ftate of a calx. Dr. Hales has not

not difcarded this opinion; but he has moreover advanced that the air contributed to produce that effect, and that to it, in fome degree, muft be attributed the augmentation in the weight of metallic calxes. He founded this opinion, on his having obtained from 1922 grains of lead, only feven inches of air, by diftillation, whereas an equal quantity of minium afforded him 34 inches.

The Doctor has alfo remarked that the phofphorus, or rather the pirophorus of M. Homberg, diminifhed the bulk of air in which it burned; that nitre could not explode in vacuo; that air was neceffary for the cryftallization of moft falts; that vegetables in fermentation, yielded, at firft, a great quantity of air which they afterwards reforbed, &c. &c. As to the diminution in the bulk of air which takes place during the burning of fome bodies, he fometimes afcribes it to the lofs of its elafticity; fometimes he feems to think that the air is really fixed and abforbed during the combuftion, and his work feems to leave this matter in fome degree of uncertainty.

However this may be, Dr. Hales finifhes his fixth Chapter of Vegetable Statics, with concluding, that the air of the atmofphere which is the fame which ferves us for refpiration, enters into the compofition of the greater part of bodies, where it exifts under a folid form, deprived of its elafticity and of moft of thofe properties by which it is diftinguifhed; that this air is, in fome manner, the univerfal bond of nature, that it is the cement of bodies; that to this is owing the great hardnefs of fome bodies, as alfo a great part of the gravity of others; that it is compofed of parts fo durable, that the action of fire, however violent, is not capable of changing them, and, likewife, that having exifted for ages in a folid and concrete form, and having paffed through trials of every kind, it is capable, under fome circumftances, of recovering all its elafticity, and becoming again an elaftic, thin fluid, wholly refembling that of our atmofphere *. And he compares air to a true Proteus, now fixed, now volatile, which fhould be

* Dr. Hales excepts the cafe of vitrification, " when with the vegetable falt and nitre in which the air is incorporated,

be adopted among the chemical principles, and poſſeſs a rank which has, hitherto, been denied to it.

CHAPTER IV.

THE OPINION OF BOERHAAVE ON THE FIXATION OF AIR IN BODIES, AND ON ELASTIC VAPOURS.

THE celebrated Boerhaave, to whom we are indebted for an excellent Treatiſe on the Elements, does not ſeem to have been always of one uniform opinion, as to the combination and fixation of air. Sometimes he appears to deny that air can be combined with bodies ſo as to contribute to the formation of their ſolid parts; ſometimes he ſeems to adopt the contrary opinion, and to inliſt under the banner of Dr. Hales. In ſhort, on reviewing what this author has

porated, he ſuppoſes, it may perhaps ſome of it, with other chymical principles, be immutably fixt." Hales's Vegetable Statics, p. 316.

has ſaid in different paſſages of his works, one may plainly diſcern, that the publication of Dr. Hales's experiments had induced him to change his opinion, and that he adopted, to a certain degree, the ſyſtem of the fixation of air in bodies; but it is alſo evident, that this theory did not appear to him ſufficiently proved to oblige him to retract, from his works, what he had ſaid on the contrary ſide of the queſtion.

However this be, it is at the end of his treatiſe on air, that he explains himſelf in the moſt particular manner, on the opinion of Dr. Hales; we find there a train of experiments, made with that accuracy which characteriſes the works of Boerhaave, on the air diſengaged from bodies by combination, and we cannot but agree alſo, that the apparatus he made uſe of had ſome advantage over that of Dr. Hales. This advantage conſiſted in his not permitting the factitious air to have any immediate communication with the ſurface of the water. We have already obſerved, that for want of ſuch precaution, it is poſſible to fall into conſiderable errors, as to the quantity of air produced or abſorbed.

It

It was in the vacuum of an air pump, and under a receiver of known dimenſions that Boerhaave always operated; he took care exactly to pump out all the air, before he made the mixture; and he afterwards judged of the quantity of air produced, by a good barometer. By the aid of this apparatus he diſcovered that one drachm and half of crab's eyes, diſſolved in one ounce and a half of diſtilled vinegar, yielded eighty one inches of air: that one drachm of chalk yielded, with two ounces of the ſame acid, one hundred and fifty one inches. That the combination of the ley of tartar, whether with vinegar, or vitriolic acid, alſo furniſhed him with a very conſiderable quantity; that there were other combinations, ſuch as the ſolution of iron by the nitrous acid, which though attended with conſiderable effervefcence, did not produce any ſeparation of elaſtic fluid in vacuo; and laſtly, that the ſmoaking ſpirit of nitre and oil of caraway yielded a ſeparation of air ſo very conſiderable, that the experiment was dangerous, unleſs the operator had the precaution to employ exceedingly large veſſels, and to make his trials with only very ſmall quantities.

These experiments are followed by ſome accounts of the ſeparation of air which takes place in combuſtion, in fermentation, in putrefaction, and in ſome diſtillations; and the author has finiſhed his Treatiſe with the following reflections, which, I think, ought to be tranſcribed at large.

" All theſe different methods, which reſemble one another in their acting by means of fire, prove that elaſtic air enters into the compoſition of bodies, as a conſtituent, and, even, very conſiderable part. If any one ſtill doubt, he muſt at leaſt acknowledge, that by means of fire, a matter may be extracted from every known body, which, when once ſeparated, is fluid and elaſtic; which is compreſſible by weights; which contracts in the cold, and dilates either by heat, or by the diminution of the weight with which it is preſſed: but when what we call elaſtic air is ſeparated from bodies with which it has been mixed, we are unacquainted with any other properties of it than the above. It muſt then be acknowledged that fire ſeparates, from all bodies, an elaſtic vapour, and, conſequently, that this aërial

" matter

" matter refides in bodies, but in fuch a manner " as not to produce the effects of air, as long as " it is combined and united with them; but " that whenever it is detached, and joins itfelf " with other parts fimilar to itfelf, it then re- " fumes its former nature, and continues to be " air, till, divided again into its elements, it re- " unites with other parts of a different kind, " where it may remain quiet for fome time, and " form one and the fame mafs without, however, " lofing any of its former nature; for it fhews it- " felf always the fame, whenever it is freed from " the bonds which retain it, and is joined with " other aërial particles of the fame nature. It is, " then, unchangeable in all thefe different circum- " ftances; difengaged from a body, it becomes " true air, as before its union, and difpofed to " join with other matter, fo as to form a new body, " fuch as it had juft quitted. No art demon- " ftrates this refolution and compofition more " clearly than chemiftry; and I fhould have " given feveral examples of it, had I not lately " read the celebrated Dr. Hales's excellent trea- " tife on vegetable ftaticks; in the fixth chapter " of that book, the author has collected with

" much

" much labour and juftnefs, and has related in " the beft poffible order, the experiments which " have been made on this fubject, and he has " exhaufted the matter. To thefe I refer my " readers, they will there fee how art has arrived " at the power of unveiling nature."

CHAPTER V.

THE OPINION OF M. STALH, ON THE FIXATION OF AIR IN BODIES.

ALTHOUGH fome of M. Stalh's writings are pofterior to the publication of Dr. Hales's experiments, he does not appear to have adopted any part of his fyftem of the fixation of air in bodies. There is not even any appearance, of his having been acquainted with the Doctor's experiments. At leaft we find the following paffage, written in the year 1731, in his work entitled, *Experimenta, obfervationes et animadverfiones*, §. 47, " *Elaftica illa expanfio* " *aëri,*

"*aëri, ita per eſſentiam propria eſt, ut nunquam*
"*ad vere denſam aggregationem nec ipſe in ſe, nec*
"*in ullis mixtionibus coiviſſe ſentiri poſſit.*"

CHAPTER VI.

M. VENEL's EXPERIMENTS ON THE WATERS IMPROPERLY CALLED ACIDULOUS, AND ON THE ELASTIC FLUID CONTAINED IN THEM.

THUS the impreſſion which Dr. Hales's Treatiſe had made upon the learned at the time of its publication, did not produce in the theory of Phyſics and Chemiſtry, the immediate reformation which it was reaſonable to expect. His experiments formed, as it were, only the connecting ſtones, which it was neceſſary to join to the edifice of phyſical knowledge.

M. Venel, the preſent Profeſſor of Chemiſtry in the Univerſity of Montpellier, laid the firſt foundation of this enterpriſe in two Memoirs read in 1750, before the Royal Academy of Sciences; they may be found in the ſecond volume of *Mèmoires préſentés par les Sçavans étrangers.* The intention of theſe Memoirs is to prove, contrary to the opinions of the ancients, and to thoſe of Hoffman and Slarre, that the Seltzer waters, and moſt of thoſe which we have been accuſtomed to denominate acidulous, are neither acid nor alkaline; that they owe their ſharp, briſk and penetrating taſte, and the bubbles which mount to their ſurface, and which imitate the appearance of champagne, of beer or of cyder, to a conſiderable quantity of elaſtic fluid or air combined with theſe waters in a ſtate of diſſolution; M. Venel proceeded ſo far as to be able to ſeparate this air by ſimple agitation, to receive it in a moiſtened bladder, and to meaſure the quantity. Whatever means he made uſe of to obtain the ſame end — whether he employed the air-pump, heat, or Dr. Hales's apparatus, the reſult was conſtantly the ſame, and he uniformly obſerved that Seltzer water contained about a fifth of its bulk of fixed air.

When the Seltzer water has been deprived of the

the air which it contained in a ſtate of diſſolution, whether by agitation, heat, or any other means, it no longer poſſeſſes the properties which conſtituted it acidulous; inſtead of the pungent taſte which it before impreſſed, it becomes flat, and ceaſes to ſparkle; in ſhort, it returns to the ſtate of common water, only, as M. Venel obſerves, it contains a ſmall quantity of ſea-ſalt.

M. Venel thought proper to purſue his reſearches ſtill further, and having proved that the Seltzer water owed its properties to the air which it contained, he endeavoured to combine air with water, and to form anew an aërial water ſimilar to that of Seltzer. The following are nearly the reflexions by which he was guided in his experiments.

Air, ſays he, is ſoluble in water*; this is demonſtrated by the example of briſk wines and Seltzer water; but the particles of this air are

* M. Venel has always ſuppoſed the elaſtic fluid contained in mineral waters to be common atmoſpheric air; it will appear in the ſequel how far this opinion is to be embraced.

to be confidered as having a ftronger affinity with each other, than with the fluid employed to diffolve them; from whence it follows that the folvent cannot, of itfelf, have fufficient force to deftroy the aggregation of the air; and that a previous condition to its diffolution is the breaking of this aggregation.

No method appeared to M. Venel more likely to produce this effect, than to decompofe falts in the water intended to diffolve them. Thus he might excite an effervefcence, and confequently let loofe a large quantity of air; and this air being in a ftate of abfolute divifion, it would therefore be under circumftances the moft favourable to its diffolution.

M. Venel was ftill further confirmed in this opinion, by the following mode of reafoning. Effervefcence is, according to him, nothing elfe than a true precipitation of air: Two bodies, when uniting, produce an effervefcence from this caufe only, that they have a greater affinity with each other, than the one or both of them have with the air to which they were united; but it

is

is well known that in many chemical precipitations, if the operation be made in much water and the precipitate be ſoluble in that fluid, it will be rediſſolved as faſt as it is precipitated: The ſame thing ſhould happen to air under ſimilar circumſtances.

After all theſe reflexions M. Venel diſſolved in a pint of water two drachms of foſſile alkali, to which he added an equal quantity of marine acid; being previouſly aſſured of the following facts: 1ſt. that this was preciſely the proportion neceſſary to a perfect ſaturation: 2dly, that it was alſo the ſame which he had obſerved in the Seltzer water. He had the precaution to make uſe of a veſſel with a narrow neck, and to prevent the eſcape of the air, by diſpoſing the ingredients in ſuch a manner, that they could not communicate with each other till after the bottle was corked. By this method he was able to compoſe a water, not only analogous to that of Seltzer, but much more ſtrongly impregnated with air. We have already ſeen that the Seltzer water does not contain more than a *fourth* or *fifth*, whereas M. Venel could introduce into his factitious water nearly *half* its bulk of air.

These experiments of M. Venel, however, were infufficient to explain a fingular phenomenon which feemed contradictory to his opinion: M. Hoffman had obferved that the waters of Troplitz and Piperine in Germany, as well as many others which are fpirituous or acidulous, do not contain any portion of faline matter: it was therefore evident that thefe waters do not acquire their air by the method employed by M. Venel; and it plainly follows that in fuch cafe, *his* procefs is not that of nature.

The explanation of this phenomenon was referved for Meffrs. Cavendifh and Prieftley; but previous to my recital of their experiments, which are much more modern, the order of facts obliges me to give an account of thofe made by Dr. Black, Profeffor in the Univerfity of Glafgow. This author may be truly regarded as the perfon who firft introduced Fixed Air into Chemiftry.

CHAPTER VII.

Dr. BLACK's THEORY concerning the FIXED or FIXABLE AIR contained in CALCAREOUS EARTHS, and the PHENOMENA produced in them by depriving them of this AIR.

MAGNESIA, calcareous earth, and all the earths in general which are reduced to quick-lime by calcination, conſiſt only, according to Dr. Black, of a combination of a large quantity of fixed air with an alkaline earth, naturally ſoluble in water. By the term *fixed air* Dr. Black means a ſpecies of air different from common elaſtic air, diſperſed nevertheleſs in the atmoſphere. He informs the reader, that it may appear wrong to make uſe of that name, but he rather choſe to retain a word already known in natural philoſophy, than to invent a new one, while we were imperfectly acquainted with the nature and properties of the ſubſtance ſo denominated.

Fixed air, according to Dr. Black's experiments, may be ſeparated, in two different ways, from calcareous earth; either by fire, or by diſſolution in acids. The calcareous earth, in the firſt caſe, viz. by calcination, loſes more than half its weight---The remainder is an earth, abſolutely deprived of air, and which, conſequently, no longer effervеſces with acids. Lime (which is the name we uſually give to calcareous earth in this ſtate) owes its cauſticity, according to Dr. Black, to the great affinity which it has with the air of which it has been deprived by calcination; thus, as ſoon as it is applied to any animal or vegetable ſubſtance, it attracts eagerly the air contained in it, effects a decompoſition; and it is this decompoſition or kind of deſtruction which is improperly denominated burning or cauteriſing.

This property, which is poſſeſſed by lime, of attracting the air from other bodies, furniſhes us with the means of communicating cauſticity to

fixed

fixed and volatile alkaline ſalts. If to a ſolution of fixed alkali, be added a certain quantity of lime, it attracts to itſelf the whole of the air contained in the alkali; at the ſame time it loſes all the conſtituent properties of lime; it becomes capable of effervеſcing with acids, and inſoluble in water; in ſhort, it returns to the ſtate of common calcareous earth: on the other hand, the fixed alkali which has been deprived of its air, no longer ferments with acids, is incapable of being cryſtallized, becomes cauſtic, and when dried by fire, and reduced to a concrete form, it is called the *ſtronger common cauſtic.*

The ſame thing happens to the volatile alkali. If we diſtill ſal ammoniac with chalk, we obtain a volatile alkali in a ſolid form, which effervеſces with acids; but if inſtead of chalk, we employ a calcareous earth deprived of its air, otherwiſe called lime, the volatile alkali, as faſt as it is ſeparated, is robbed of its air by the lime, and paſſing into the receiver in a fluid form, is a cauſtic volatile alkali which neither effervеſces with acids, nor can be formed into cryſtals. It appears from Dr. Black's experiments, that the attach-

attachment of fixed air is not equal to all bodies; but that it has a greater affinity with calcareous earth than with fixed alkali, with fixed than with volatile alkali, &c.

Another method of detaching, from calcareous earth, the air with which it is combined, is by the addition of acids. If we diſſolve lime-ſtone or chalk in any acid, a briſk efferveſcence, or which is the ſame thing, a conſiderable ſeparation of fixed air, enſues; the earth, which has a greater affinity with the acid than with the air, abandons the latter, which, recovering its elaſticity, immediately eſcapes, and is diſſipated and mixed with the common air of the atmoſphere. If, afterwards, we precipitate the earth from this ſolution, we may obtain it, as we pleaſe, either in the form of chalk or of lime; of chalk if we employ a common alkali for the precipitation, but of lime if it be effected by means of a cauſtic alkali, or alkali deprived of its air. What is here particularly to be remarked is, that, as Dr. Black informs us, the lime-ſtone loſes nearly the ſame weight in this proceſs as by calcination; and that it recovers its former weight

weight when it has been precipitated in the form of a calcareous earth, that is to ſay, with all its air.

Dr. Black explains on the ſame principle, why lime is not totally ſoluble in water; why the part which is diſſolved is ſo eaſily converted into a pellicle, inſoluble in water, and known by the name of *Cream of Lime.* Calcareous earths, according to his opinion, have a ſtronger affinity with air, than they have with water; conſequently, on adding lime to water, one part of the lime will attract from the water the fixed air contained by the latter, and be precipitated as a calcareous earth; but, at the ſame time, another portion of the lime, not finding a ſufficient quantity of fixed air to ſaturate it, will be diſſolved in the water, and form lime-water; if we then expoſe this water to the air, preſently the particles of lime neareſt to the ſurface attract the fixed air floating in the atmoſphere; they again become inſoluble, and collecting together on the ſurface, form an inſoluble pellicle, which no longer retains the properties of lime, nor differs from calcareous earth. A proof of the truth

of

of this theory is, that the reduction of the lime into a calcareous earth may be prevented, by keeping the lime-water in bottles well ſtopped, ſo as to prevent any contact with the circumambient air.

Dr. Black has alſo obſerved, that Magneſia, the baſis of Epſom ſalt, has the property of ſweetening lime-water; and, therefore, that fixed air has a ſtronger affinity with calcareous earth that with the baſis of Epſom ſalt. Laſtly, from all theſe experiments, Dr. Black concludes, that we may make the following alterations in the column of acids, in Mr. Geoffroy's Table of Affinities; and that we may alſo add a new column, conſidering alkaline ſubſtances in their natural ſtate, and deprived of their fixed air, as follows:

ACIDS.	FIXED AIR.
Fixed Alkali.	Calcareous Earth.
Calcareous Earth.	Fixed Alkali.
Volatile Alkali.	Magneſia.
Magneſia.	Volatile Alkali.

The

The limits of an abſtract do not permit me to enter here into a detail of the great number of intereſting experiments relative to the diminution of weight, which alkalis ſuffer after they have been diſſolved in acids; to the manner of rendering alkalis cauſtic by fire, &c.

I must not however omit to add, before this article be concluded, that Dr. Black ſuſpected, that the fixed air of alkaline ſalts unites itſelf to metals, during their precipitation from acids; and that to this cauſe, the augmentation of weight, obſervable in theſe precipitates, and perhaps the ſurpriſing effects of the pulvis fulminans, may be attributed*.

CHAP.

* It is thought neceſſary to acquaint the reader, that the theory of fixed air, had not acquired the perfection and conſiſtence which is given to it in this article, when it came from the hands of Dr. Black. It did not arrive to this point till after the publication of M. Jacquin's work, of which we ſhall ſhortly give an account. I judged it proper to make this remark here, not with a view to take in the leaſt from the ſentiments of reſpect and admiration which

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CHAPTER VIII.

ON THE ELASTIC FLUID SEPARATED FROM GUN-POWDER; BY THE COUNT DE SALUCES.

WHILE Dr. Black was publiſhing in England, the theory of which we have juſt now given an account, the Count de Saluces employed himſelf at Turin, in ſome very intereſting inquiries into the nature of the elaſtic fluid which is detached from gun-powder, during its detonation. He has diſcovered, that this fluid,

which are due to the deſert and genius of Dr. Black, to whom, without equivocation or diviſion, the merit of the invention belongs, but to render to M. Jaquin the juſtice that is due to him, and to avoid a remonſtrance on his part, which would have been well founded. Further, we ſhall ſee preſently, that M. Jaquin has departed from the opinion of Dr. Black, ſo far as to ſuppoſe the fixed air to be the ſame with that which compoſes our atmoſphere.

fluid, when at liberty, occupies a ſpace two hundred times greater than that of the powder from which it has been diſengaged. A number of experiments convinced him, that this fluid was, like the air of the atmoſphere, elaſtic; that, like the latter, it was compreſſible in proportion to the weight with which it was loaded; but that it differed, notwithſtanding, in theſe particulars, that it extinguiſhed the flame of a candle, and alſo was mortal to thoſe animals who breathed it. He attempted to filter this air, through linen or through gauſe, well impregnated with a ſolution of fixed alkali; there remained in the filtre, a ſmall quantity of coaly matter of fixed alkali, and ſome traces of vitriolated tartar. The air, after this operation, had loſt all its noxious properties, and did not ſeem to differ in any reſpect from common air.

Another method which the Count points out to reſtore to the air, ſeparated from gun-powder, all the properties of common air, is to keep it for twelve hours in a degree of cold, equal to that in which water freezes. He aſſures us, that he had repeated the ſame experiment on air detached

tached by the effervefcence of an acid with an alkaline fubftance, and with the fame fuccefs.

Independent of thefe experiments, which tend effentially to the object which the Count de Saluces had in view, his memoirs contain feveral others, which throw light on the theory of the combination of air with bodies. He obferved, that the air difengaged from effervefcing fubftances, for the moft part, extinguifhed flame; that what was feparated in the combination of the volatile alkali with vinegar, was an exception to this general rule; that the nitrous acid, when mixed, in vacuo, with a fixed alkali, produced no air; that this combination remained chiefly deliquefcent, as long as it was continued in vacuo, but that it cryftallized when it had been expofed for fome time to the air. This experiment, joined to thofe of Dr. Black on the cryftallization of fixed alkali, feems to give occafion to fufpect, that the combination of air is neceffary to the formation of faline cryftals.

The Count de Saluces further obferves, that gun-powder explodes, in air however infected, whether

whether it be by the burning of ſulphur, whether candles have been extinguiſhed in it, or whether it have been diſengaged by the exploſion of another portion of the ſame powder. He then ſhews that the phœnomena of the pulvis fulminans are the ſame with thoſe of gun-powder; that they are occaſioned by the ſeparation of the ſame elaſtic fluid; but, what is very ſingular, is that the quantity of that fluid which is detached from the pulvis fulminans is leſs than what is diſengaged from gun-powder; from whence the Count concludes that the effects are leſs in proportion to the quantity of air ſeparated, than to the rapidity, and if I may be allowed the expreſſion, the *inſtantaneiety* of the ſeparation. I take no notice here of an infinite number of intereſting facts with which the Count's memoir is replete, becauſe they are rather foreign to my ſubject; I ſhall only add, before I finiſh this article, that the Count de Saluces only admits of one ſpecies of air, in which opinion he differs eſſentially from that of Dr. Black.

CHAPTER IX.

Dr. MACBRIDE's APPLICATION of Dr. BLACK's DOCTRINE of FIXED AIR to the EXPLANATION of the principal PHENOMENA of the ANIMAL ŒCONOMY.

HITHERTO the exiſtence of fixed air, and its combination with bodies was but a phyſical opinion, founded on particular experiments; but no phyſiologiſt, ſince Van Helmont, had adopted it. M. Haller was the firſt who, from Dr. Hales's experiments, has inſtructed us, that the air was the real cement of bodies, that this was the principle which fixing itſelf in the ſolids and fluids ſerved as a bond to the elementary parts, and united them to each other.

Videtur aer vinculum elementorum primariam conſtituere, cum non priús ea elementa a ſe invicem diſcedant

discedant quam aer expulsus fuerit. Haller, *Elementa Physiologiæ*, Tit. 1. Cap. 1.

Gluten præstat verum moleculis terreis adunandis, ut constat exemplo calculorum lapidum, aliorum corporum durorum; in his omnibus solvitur tunc demum partium vinculum quando aer educitur. Ibid. Scel. 244.

A very numerous and accurate series of experiments appeared in 1764 in support of this doctrine. Dr. Macbride of Dublin, the author of the essays which contain them, possesses too distinguished a rank among the writers on this subject, for us to omit entering into a detail of the important facts in natural philosophy and physiology, the discovery of which we owe to him.

It is demonstrated by Dr. Macbride's experiments, that fixed air is separated, not only from effervescing substances and from vegetable matter during fermentation, but also from all animal substances as soon as they begin to putrefy; and to prove the very great facility with which this air is capable of uniting itſelf, either with lime or with the fixed or volatile alkalis; he made uſe of what is now called Dr. Macbride's apparatus, though the original idea is due to Dr. Black. We ſhall endeavour to deſcribe the manner in which he operated. He placed ſucceſſively in a bottle, ſaline ſubſtances in the act of effervefcence, vegetable ſubſtances in fermentation, and laſtly, animal ſubſtances which had begun to putrefy; he obliged the air which was ſeparated from them to paſs through a bended tube, into a bottle or flaſk which ſucceſſively contained lime water, and the fixed and volatile alkalis in a cauſtic ſtate. As ſoon as the fixed air, detached from theſe different bodies, came into contact with the ſurface of the lime water, it became turbid, and ſoon after the earth gradually ſubſided in the form of calcareous earth, viz. poſſeſſed of all its air and without any ſigns of cauſticity. So likewiſe the fixed and the volatile cauſtic alkalis, in proportion as they became combined with the fixed air, recovered their property of effervefcing with acids; and as ſoon as they were in a ſufficiently concentrated ſtate, reaſſumed their ſolid form and cryſtallized

in

in the bottle. This experiment ſhews us, that if the fixed vegetable alkali has not the property of cryſtallizing, it is becauſe, being prepared by a ſtrong fire, we generally procure it deprived of ſome part of the air which it naturally poſſeſſes. If therefore we reſtore to it this quantity of air, it at the ſame time recovers its property of cryſtallizing. We find the firſt traces of this laſt diſcovery in Dr. Black's Memoirs.

The different experiments made by Dr. Macbride on the great quantity of fixed air which is ſeparated from animal ſubſtances during their putrefaction, led him to conclude, that on the preſence of this elaſtic fluid or fixed air in fleſh depend its firmneſs, its coheſion and ſtate of ſoundneſs; that in proportion as the fixed air is ſeparated from it by the fermentation, its texture is deſtroyed, its conſtituent parts become diſunited, and ſeparate to reunite in a different manner, and to form new combinations very different from the former.

It is eaſily perceived that this doctrine is nearly the ſame as that which had been taught by

Van Helmont; but one important difcovery, fuppofing the fact to be fufficiently proved, * belongs wholly to Dr. Macbride, viz. that flefh which is half putrid and has loft a portion of the fixed air which entered into its compofition, may recover its former fweetnefs, by reftoring to it the fixed air of which it had been deprived: to produce this effect, it will be fufficient to expofe it to the vapours of any fermenting matter, or rather to a current of fixed air from an effervefcing mixture; in fhort, to introduce the fixed air by any means whatfoever.

Dr. Macbride applies thefe different difcoveries to explain the phenomena of the theory of digeftion; he fhews that all the alimentary mixtures which we commonly ufe, are fufceptible of fermenting in a fhort time; that animal and vegetable fubftances when mixed, have a greater aptitude to ferment, than either of thefe fubftances poffefs feparately; and that from all the alimentary mixtures, on which he has made a numerous train of experiments, he always feparated a confiderable quantity of fixed air.

Dr.

* Vide Henry's Experiments and Obfervations, page 114 & feq.

Dr. Macbride ſuppoſes that the ſame ſeparation muſt take place in the ſtomach of an animal; but what becomes of this fixed air? He imagines that it is either abſorbed and combined with the chyle, and paſſes, in that ſtate, into the circulation of the blood, or rather that it is abſorbed in the inteſtinal canal, by particular veſſels, adapted to this kind of ſecretion: in either caſe the air is carried off either by perſpiration or urine. This theory leads Dr. Macbride to engage in a long train of experiments on the greater or leſs quantity of fixed air, contained in the different animal ſecretions. Lime water appeared to him to be the proper teſt in this inquiry; for as lime has a great affinity with fixed air, whenever any liquor containing that air is mixed with it, it greedily abſorbs it, ſaturates itſelf with it, and now no longer ſoluble, it is precipitated and ſubſides in the form of a calcareous earth. By this mode of trial, Dr. Macbride diſcovered that blood, newly drawn, contained a great quantity of fixed air; purſuing his experiments ſtill futher he diſcovered that this air reſided in the red part of the blood while the ſerum was free from it. By experiments of a ſimilar kind, he alſo was informed that the ſweat and urine contained much fixed air, whereas on the contrary the bile and eſpecially the ſaliva, ſo far from containing it, had a tendency to abſorb it.

It would be too tedious, here to relate the ſeveral experiments made by Dr. Macbride on the fermentation of alimentary mixtures, and on the means of accelerating or retarding fermentation. Let it ſuffice to ſay that they led the author to the moſt important reflections on putrid diſeaſes, and the ſea-ſcurvy, which according to Dr. Macbride's theory of putrefaction, owe their origin to the privation of that certain quantity of air which is neceſſary to a ſtate of ſalubrity. It is alſo obſervable that the diet, which is moſt unſuitable in theſe diſeaſes, is that compoſed of animal food, which, by Dr. Macbride's account, yields much leſs air by fermentation than vegetables; the method of cure on the contrary conſiſts in the uſe of a vegetable diet, and of all thoſe ſubſtances which are capable of furniſhing an abundance of fixed air. On theſe principles Dr. Macbride recommends the uſe of malt as a cure for the ſea-ſcurvy; this ſubſtance furniſhes a decoction very proper for

fermentation

fermentation and which ſupplies a larger quantity of fixed air than any other vegetable body. With the ſame view he preſcribes ſugar and water, and other ſimilar liquors.

THE antiſeptic quality which is ſo univerſally known to belong to acids, Dr. Macbride attributes ſolely to the particular property which they have of uniting with the alkaline particles of the putrefying ſubſtance, and thereby neutralizing them; but as a remedy, he eſteems them to be rather palliative than curative, becauſe they do not like fixed air, reſtore the part to its natural ſtate.

INDEPENDENT of the experiments already recited, which are eſſentially connected with Dr. Macbride's theory, his Treatiſe contains many others of which the following are the principal:

1ſt. THE ſeparation of fixed air from fermentative mixtures is accelerated in Boyle's vacuum.

2dly. CALCAREOUS earths have the property of haſtening putrefaction.

3dly. Lime produces a very particular effect on animal ſubſtances. It decompoſes them by abſorbing the fixed air which they contain, and thereby produces an effect, in ſome meaſure, anologous to that of putrefaction.

4thly. The union of oil and fixed alkali takes place according as the latter is deprived of its air. If the vapours, ariſing from an efferveſcing or a fermentative mixture, be ſuffered to paſs into a ſolution of ſoap, the fixed air which is ſeparated, combines itſelf gradually with the fixed alkali of the ſoap, and the oil being ſet at liberty, ſwims on the ſurface.

5thly. Ardent rectified ſpirits abſorb fixed air when expoſed to it.

The Doctor has alſo proved that the volatile alkali, which is diſcharged from animal ſubſtances, in the progreſs of putrefaction, is ſometimes in its natural ſtate or ſaturated with air, ſometimes, on the contrary, wholly diveſted of its air, and in a cauſtic ſtate. For example, he diſcovered by a ſeries of experiments, that putrefied

putrefied blood, as well as the ſpirit drawn from it, effervefced with acids; whereas bile, though equally putrid, and alſo the liquor which runs from putrefied fleſh, did not produce any effervefcence; neither did the ſpirit diſtilled from them differ, in this reſpect, from the liquors themſelves.

From all theſe experiments, Dr. Macbride concludes that fixed air is an elaſtic fluid, very different from atmoſpheric air; that the former may be introduced without danger into the inteſtinal canal, as well as into other parts of the animal economy, without occaſioning any diſorder; whereas atmoſpheric air under the ſame circumſtances, would be fatal in its effects: but yet, on the contrary, it is impoſſible for animals to live without reſpiring continually the fluid which forms our atmoſphere, whereas fixed air inſpired into the lungs, is a ſubtle poiſon occaſioning immediate death: That fixed air very eaſily unites with lime or with alkaline ſalts, whereas we cannot, by the ſame methods, combine them with the air of our atmoſphere. Laſtly, he adds, that fixed air is found to be diſperſed difperfed in our atmofphere, fince lime and cauftic alkalis, in time, lofe their diftinguifhing properties, and acquire that of effervefcing with acids. Thefe conclufions are nearly the fame as thofe of Van Helmont.

CHAPTER X.

EXPERIMENTS MADE BY THE HONOURABLE MR. CAVENDISH ON THE COMBINATION OF FIXED AIR WITH DIFFERENT SUBSTANCES.

SOON after the publication of Dr. Macbride's Treatife, Mr. Cavendifh communicated to the Royal Society of London fome new experiments which tended equally to confirm Dr. Black's doctrine; they are to be found in the Philofophical Tranfactions for the years 1766 and 1767. Mr. Cavendifh has proved that the quantity of fixed air contained in fixed alkali when fully faturated with air, is five-twelfths of its weight, and feven-twelfths in volatile alkali, which

which great quantity of air is ſometimes the cauſe of the briſk effervescence which appears in a calcareous earth after it has been diſſolved in the nitrous acid and precipitated by an alkali thus ſaturated with air; for in fact, as the precipitant furniſhes more air than the precipitated matter can abſorb, there is neceſſarily a portion at liberty, which, recovering its elaſticity, occaſions the effervescence.

Mr. Cavendish farther ſhows that water is capable of abſorbing a volume of air more than equal to itſelf; that this quantity is proportionably greater as the water is colder, and is compreſſed by a heavier atmoſphere; that water thus impregnated with fixed air has an acidulous, ſpirituous, and not diſagreeable taſte; and laſtly, that it has the property of diſſolving calcareous earth and Magneſia. It follows, as a conſequence of this property of water impregnated with fixed air, that if after precipitating the lime from lime-water by throwing fixed air into it, ſtill more of the ſame air be added, the water becomes capable of rediſſolving a part of the earth which had been precipitated.

Water impregnated with fixed air has alſo the property of diſſolving almoſt all the metals, and eſpecially iron and zinc; a very ſmall quantity of theſe metals is ſufficient to communicate to water their taſte and virtues *.

These circumſtances ſeem to explain, in the moſt natural manner, how the moſt pure diſtilled water diſſolves iron, as appears from Mr. Monet's obſervations, and why that combination takes place more readily in cold than in hot water: the reaſon is this, the water acts on the metal only in proportion to the fixed air which it contains; and it has been already obſerved that it contains leſs in proportion to its heat. For this reaſon we are not able to obtain the leaſt particle of vitriol from moſt of the mineral, ferruginous waters.

We are alſo informed by Mr. Caveniſh, that fixed

* Though M. Lavoiſier has placed this diſcovery here, he acknowledges it does not belong to Mr. Cavendiſh. It is, in reality, the property of my very ingenious friend Mr. Lane of Alderſgate-ſtreet; and was made in conſequence of a converſation with Dr. Watſon, junior. Vide Philoſophical Tranſactions, Ann. 1769, and Sir John Pringle's Diſcourſe on the different kinds of Air, page 11. T. H.

fixed air may be combined with ſpirit of wine and with expreſſed oils, but that theſe ſubſtances do not, in other reſpects, obtain any new properties by the union; that the vapour of burning charcoal occaſions a remarkable diminution of air, and, that at the ſame time, a quantity of fixed air is produced in the operation which is capable of being abſorbed by ſoap ley. And to conclude, Mr. Cavendiſh is the firſt who has remarked, that a ſolution of copper in ſpirit of ſalt, inſtead of yielding inflammable air, like that of iron or zinc, afforded a particular ſpecies of air, which loſt its elaſticity as ſoon as it came into contact with water.

CHAPTER XI.

M. MEYER's THEORY CONCERNING THE CALCINATION OF CALCAREOUS EARTHS, AND THE CAUSE OF CAUSTICITY IN LIME AND IN ALKALIS.

WHILE the doctrine of fixed air was peaceably eſtabliſhed in England, a formidable opponent to it aroſe in Germany. Nearly

Nearly at the ſame time that Dr. Macbride publiſhed, in Engliſh, the Eſſays of which we have juſt given an account, a very elaborate Treatiſe was publiſhed, written in German, by M. Meyer, Apothecary at Oſnabruck, entitled, *Eſſays in Chemiſtry, on Quick-lime, the Elaſtic and Electric Matter, Fire, and the univerſal primitive Acid.* This Treatiſe contains a great number of experiments, for the moſt part accurate and true, from which the author has deduced conſequences totally oppoſite to thoſe drawn by Doctors Hales, Black and Macbride. There are few modern books of chemiſtry which diſplay more genius than this of M. Meyer; and if his ideas were to be adopted, the conſequence would be nothing leſs than a new theory directly contrary to that of Stalh, and of all the modern chemiſts.

Mr. Meyer, firſt examines the nature of the calcareous ſtones of the ſparry kind, and of ſubſtances proper to make lime. He remarks that theſe ſubſtances are rarely pure, that they are commonly mixed with ſand and other foreign matter; but that the part really proper to make lime, is nothing more than a pure earthy alkali,

inſoluble

insoluble in water, susceptible of combination with acids, in which it dissolves with effervescence, &c. He observes, that as soon as these substances have been exposed, a sufficient time, to the action of a strong fire, they suffer a large quantity of water to escape; that after this operation they come out with the property of being wholly soluble in water, and of no longer effervescing with acids. From these new properties M. Meyer concludes, that the lime, while in the fire, has been neutralized by a particular acid, to which, as a medium, its solubility in water is due, and whose union with it has deprived it of its property of effervescing with acids. To confirm this theory, Mr. Meyer poured into lime-water, drop by drop, some lixivium of fixed alkali. The lime-water presently became turbid, and the lime subsided under the form of a calcareous earth, insoluble in water as before its calcination; the alkali, on the other hand, had acquired the causticity of the lime, and a part of its other properties. From whence Mr. Meyer concludes, that the acid which was united to the lime, and rendered it soluble, has more affinity with fixed alkali than with lime; that it abandons the latter, and unites with the fixed alkali. The ſame thing happens when lime-water is precipitated by the volatile alkali, or on ſeparating, by means of lime, the volatile alkali from ſal ammoniac: in all theſe caſes, the acid of the lime neutralizes the ſalt, renders it cauſtic, uncriſtalizable, and deprives it of the property of effervеſcing with acids. The acid ſubſtance, which the lime thus attracts in the fire, Mr. Meyer calls *acidum pingue*; he ſuppoſes it to be a ſubſtance nearly approaching to that of fire and of light; that it is by the aid of this acid, that lime unites with oil, diſſolves ſulphur, &c. Laſtly, it is Mr. Meyer's opinion, that the *acidum pingue* enters very abundantly into the compoſition of vegetables and animals; that it is this which eſcapes from charcoal when burning, from wood when it conſumes, &c.

Mr. Meyer goes on to point out its combination with a great number of bodies; he ſuppoſes it to exiſt in metallic calces, in minium, and that it may be made to paſs from them into either the fixed or volatile alkalis, which thereby acquire a ſtate of cauſticity. It is principally in this

this article that Mr. Meyer's theory ſeems to have the advantage over the Engliſh ſyſtem. In fact the theory of the acidum pingue explains in the moſt natural and moſt ſimple manner the augmentation in the weight of the metallic calces, their action on ſal ammoniac, the ſeparation of the volatile alkali from that ſalt by minium, litharge, and many other metallic calces. In every caſe, it is the *cauſtic* of fire, the *acidum pingue* which unites with the metals by calcination, which paſſes afterwards into the volatile alkali, and forms a ſpecies of neutral ſalt ſimilar to that which is obtained with lime.

Mr. Meyer obviates one capital objection which might be made to his theory on Dr. Black's ſyſtem. The latter has advanced, that if a pure calcareous earth be diſſolved in the nitrous acid, and afterwards precipitated by an alkali, we may have, as we chuſe, the earth precipitated either in the ſtate of calcareous earth or of lime. If it be precipitated by the common fixed, or concrete volatile alkali, it is obtained in the ſtate of calcareous earth; but in that of quick-lime, on the contrary, if it be precipitated by either the fixed or volatile *caustic* alkali. Dr. Black has explained this phenomenon in the following manner: Calcareous earth, dissolved in spirit of nitre, no longer contains any air, for it has been dissipated, during the combination, by the effervescence. If, then, the earth be precipitated from this solution by a common fixed alkali saturated with air, in proportion as the alkali unites with the acid, it abandons all its air, which is conveyed to the earth, and precipitates it under the form of a calcareous earth; if on the contrary it be precipitated by a caustic alkali, viz. by an alkali deprived of its air, the earth, not finding, in the mixture, any thing which can supply it with air, subsides in the state of lime.

The simplicity of this explanation does not at all disconcert Mr. Meyer, and he answers in a manner also quite natural. When we precipitate a solution of calcareous earth by a caustic alkali, we mix, as it were, by his account, two neutral salts together; the one a nitre with an earthy basis, the other composed of the acidum pingue and fixed alkali. A double decomposition

tion, then, ſhould take place in the mixture. The nitrous acid ſhould quit its baſis to unite with the fixed alkali, and, at the ſame time, the acidum pingue being at liberty, ſhould attach itſelf to the calcareous earth and precipitate with it in the form of lime, viz. ſoluble in water, and deprived of the property of effervеſcing with acids. But the event muſt be different when we precipitate by a common alkali; for as that does not contain the *acidum pingue*, the precipitate falls as a calcareous earth.

It would be too tedious to follow Mr. Meyer in the compariſon which he makes between the *acidum pingue* and the matter of fire, that of light, the electric, and the phlogiſtic matter. I ſhould be led, beſides, into details too extenſive for my deſign. This chemiſt, it muſt be confeſſed, gives himſelf up too much to a propenſity which all thoſe have, who believe they have diſcovered a new agent, and apply it indiſcriminately to every thing.

CHAPTER XII.

An explanation of Dr. BLACK's THEORY of FIXED AIR, by Mr. JACQUIN.

THE Englifh doctrine attacked by Mr. Meyer, foon found a defender. Mr. Jacquin, Botanical Profeffor at Vienna, publifhed in 1769, a Latin differtation in its favour intitled; *A Chemical examination of Mr. Meyer's Doctrine, of his* Acidum pingue, *and of Dr. Black's Doctrine concerning the Phenomena of Fixed Air, with regard to lime.* This differtation, though it may not have added much to what had before been done by Meffrs. Black and Macbride, may be confidered as an excellent work, on account of the method and clearnefs with which the facts are there related, the choice of experiments which it contains, the fimplicity and accuracy of the proceffes, and laftly of the right manner of philofophizing obfervable in it.

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The firſt obſervation which ſtruck Mr. Jacquin, was that lime loſes, during its calcination nearly one half of its weight. This peculiarity, which rendered Mr. Meyer's theory ſuſpected by him, engaged him to perform the calcination of lime-ſtone in cloſe veſſels. For this purpoſe, he took a ſtone-retort capable of bearing the action of fire; into this he put thirty-two ounces of lime-ſtone; he then adapted to it a large tubulated receiver, and proceeded to diſtillation.

At firſt he employed but a moderate fire, and obtained only ſome phlegm; but, preſently, having raiſed the fire, an elaſtic vapour began to ſeparate very plentifully, which continued to fly off during an hour and half, with an hiſſing noiſe, through the tube of the receiver: this vapour Mr. Jacquin ſuppoſes, to have been nothing but air. The operation being finiſhed he found no more than ſeventeen ounces of calcareous earth in the ſtate of lime, in the cucurbit, and two ounces of phlegm containing ſome ſlight traces of volatile alkali in the receiver.

The thirteen deficient ounces Mr. Jacquin attributes to the air; hence it follows, according to him, that lime-ſtone contains ſix or ſeven hundred times its bulk of air.

The object of ſeveral experiments, which are related after this, is to prove that lime-ſtone, becomes quick-lime, only in proportion to the quantity of elaſtic fluid which is diſengaged from it; and that if, for example, we draw off nothing but the phlegm, and then extinguiſh the fire, the lime-ſtone will be found in the retort, nearly in the ſame ſtate in which it was put into it. What, according to Mr. Jacquin, proves ſtill more ſatisfactorily, that it is not the deprivation of water only which conſtitutes the lime, is that if inſtead of ſtopping the operation as ſoon as the air begins to be diſengaged, we continue it ſomewhat longer, the limeſtone is reduced into quicklime on its ſurface, without being ſo interiorly.

These firſt experiments led Mr. Jacquin to ſome reflections on the manner in which air may exiſt in bodies; and he makes a diſtinction between the air which only enters into their pores

and

and that which enters into their compofition. The firft may be rendered fenfible by the mere experiment of the air pump; the latter, on the contrary is in a ftate of divifion or diffolution which does not permit it to enjoy its elafticity.

We know that lime is capable of being diffolved in water; that lime-water expofed to the air affords a pellicle which is no longer lime, but a calcareous earth which effervefces with acids. Mr. Jacquin agrees in opinion with all Dr. Black's difciples, that this fubftance is nothing more than lime which has recovered the air of which it has been deprived, and he fhews that it recovers, in proportion, the weight which it had loft by calcination. This cremor calcis calcined again lofes $\frac{13}{32}$ of its weight; its air is feparated during the calcination; in fhort, every thing declares it to have repaffed to the ftate of lime-ftone.

Mr. Jacquin, afterwards, examines the action of water upon lime; he demonftrates that it extinguifhes the lime without reftoring to it its air, becaufe we may preferve the lime under the water, as long as we pleafe, without its ceaf-

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ing to be lime, provided that we can keep the ſurface of the water from contact with the open air, otherwiſe the whole muſt be converted, ſucceſſively and in proceſs of time, into a cremor calcis. He ſhews alſo, that if we evaporate lime-water, by way of diſtillation, the earth which remains in the cucurbit is ſtill lime, and by no means calcareous earth. All theſe experiments prove further that it is not the abſence or preſence of water which conſtitutes the ſtate of lime or of calcareous earth.

Mr. Jacquin afterwards reviews all the experiments of Doctors Black and Macbride; and he has alſo added ſome, which are new, with the ſame intentions. He ſhews that every mixture of chalk or of common alkali, with an acid, produces air which has the property of precipitating lime-water; viz. of uniting with the lime which is diſſolved in the water, of converting it into a calcareous earth, rendering it inſoluble, and making it cryſtallize immediately. The air which ſeparates from lime-ſtone, during its calcination, has the ſame property.

Mr.

Mr. Jacquin oppoſes theſe experiments and thoſe of Meſſrs. Black and Macbride to Mr. Meyer's theory, and he draws, from moſt of them, objections which to him appeared inſurmountable.

Mr. Jacquin had before obſerved, that whenever the air is diſſolved, and combined with certain ſubſtances, it has, as in all the chemical combinations, 1ſt. a point of ſaturation; 2dly. a certain degree of adheſion, which is greater or ſmaller in proportion to the difference of affinity which it has with theſe different ſubſtances. He applies theſe reflections, in the cleareſt manner, to the formation of cauſtic alkalis; he maintains that lime only acts upon them by means of the greater affinity which fixed air has with it; and he alſo eſtabliſhes it as a principle, with Black and Macbride, that lime, the common cauſtic, and all cauſtics of that kind, act ſo powerfully on animal ſubſtances, only by attracting their air of which they are extremely greedy, and that as this air is eſſential to their combination, a decompoſition enſues.

Mr.

Mr. Jacquin has alſo repeated the experiments of Meſſrs. Black and Macbride on the means of making lime in the moiſt way. If calcareous earth be combined with nitrous acid in a long necked bottle, we perceive, after the effervеſcence, that the chalk has loſt nearly half its weight, viz. that it has loſt all the air which conſtituted it calcareous earth; it is then in the ſtate of lime. If we wiſh to obtain it alone in the ſame ſtate and ſeparated from the nitrous acid, we have only to precipitate it by a cauſtic alkali; the earth which remains, when waſhed from the ſalt, is a true lime ſoluble in water.

This diſſertation of Mr. Jacquin, as has already been ſaid, contains only a ſmall number of new facts, the foundation belongs, entirely, to Meſſrs. Black and Macbride; but there is to be diſcovered in his experiments much more order than in thoſe of the two Engliſh authors; and it may be regarded as a complete treatiſe on the cauſticity of lime, and of alkalis, upon the hypotheſis of Dr. Black. The fear of falling into repetitions does not permit me to avail myſelf of an infinite number of intereſting details which

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conftitute a part of the merit of this work, and which announce very great perfpicuity in his ideas, and much method in the manner of communicating them.

CHAPTER XIII.

A REFUTATION OF THE THEORY OF MESSIEURS BLACK, MACBRIDE, AND JACQUIN, BY MR. CRANS.

DEATH removed Mr. Meyer from among the learned about the time that Mr. Jaquin's work appeared; but his doctrine had already made a rapid progrefs in Germany, had been adopted by chymifts of reputation, and had begun to receive public notice in the fchools. Mr. Jacquin's work, therefore, did not meet with a friendly reception there; and in 1770, Mr. Crans, phyfician to his Majefty the King of Pruffia, publifhed againft him at Leipfick, a Latin work, entitled, *A Refutation of the chemical Examination of Meyer's Doctrine concerning the Acidum Pingue; and of Black's Doctrine concerning*

cerning fixed Air, as relative to Quick-lime, in octavo, 212 pages.

The experiments, related by Mr. Crans, are so very numerous, that I should exceed the bounds to which I have confined myself, if I were to enter into a detail of them all: I shall only endeavour to give some idea of the principal ones, and I shall especially select those which seem more directly to oppose the doctrine of fixed air.

Mr. Crans first inquires into the action of fire on lime-stone. He agrees with Dr. Black's disciples, that this substance loses a considerable portion of its weight in the fire, but he attributes this loss to the great quantity of water which it contained, and which was driven off by the force of the fire. It is also to the water reduced to vapours, or in a state of expansion, that he attributes, chiefly, the elastic separation observed by Mr. Jacquin, during the calcination of lime-stone in close vessels; but he has not produced, beyond this, any decisive proof of this assertion.

Mr. Crans is of opinion, that limestone when calcined

calcined is not deprived of the property of effervefcing with acids, as Dr. Black's followers pretend, and he quotes, on this fubject, the teftimonies of Meffrs. Duhamel, Geoffroy, Homberg, and Pott, who have all declared that lime effervefces with acids. He here adds fome experiments of his own; thefe were made on lime under different circumftances, and which had efpecially been carefully preferved from contact with the air, yet he uniformly obferved an effervefcence.

He objects on this occafion, that if lime only differ from calcareous earth in being deprived of its air, and in the great affinity which it has with it, it ought, in a fhort time to reabforb in the open air, the whole of the air of which it has been deprived, and to become again a calcareous earth; he has obferved however that lime may be preferved a long time in the air without lofing its nature; he even affures us that after a confiderable length of time, it acquires greater caufticity.

After having examined the appearances which

which limeſtone exhibits during its calcination, Mr. Crans proceeds to the ſlaking of lime. He obſerves that the ſudden ſwelling, and that conſiderable heat which is remarkable during the operation, and which is ſo natural a conſequence by Mr. Meyer's ſyſtem, is abſolutely inexplicable by Dr. Black's hypotheſis, which alſo affords no better reaſon why calcareous earth diſſolves with very little heat in the nitrous acid, whereas the diſſolution of lime in the ſame acid produces a degree of heat ſuperior to that of boiling water; that, in fine, the partiſans of fixed air are not able to give any ſatisfactory reaſon for that acrid and corroſive vapour which exhales from lime and occaſions us to cough, for the danger attending buildings newly plaſtered with lime, nor for ſeveral other effects produced by it.

Mr. Crans proceeds to examine the phenomena which lime preſents on its diſſolution in water, and during its cryſtallization. We have ſeen above that the pellicle which forms on the ſurface of lime water, when it has been expoſed, for ſome time, to the air and which is known among Chemiſts, by the name of Cremor Calcis,

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is nothing elfe, according to Mr. Jacquin, than lime which has recovered its air, and which by this union has alfo recovered the ftate of calcareous earth, viz. has become infoluble in water, and fufceptible of effervefcence; in a word, the fame as before calcination. Mr. Crans, on the contrary, fuppofes, with Mr. Meyer, that the cremor calcis is lime which has loft the cauftic principle or acidum pingue; he affures us that he has frequently feen this fubftance form itfelf at the bottom of the fluid, and not on its furface, and that it is depofited on the interior fides of the veffel, and in places where the lime could have had no contact with the air; and laftly, that it even forms while the lime-water is covered with a pellicle which cuts off all communication with the air. Moreover, all the lime, according to Mr. Crans, is not foluble in water, nor can the whole be converted into cream, which ought to follow from the principles of Dr. Black and his difciples.

Mr. Crans does not quit the fubject of lime-water till after he has dwelt a confiderable time on its properties, and he deduces from thence, almoſt every objection againſt Dr. Black's theory. Lime-water diſſolves ſulphur, camphor, and reſins, nearly in the ſame manner as ſpirit of wine; Dr. Black's diſciples, to reaſon conſiſtently, ought then to go ſo far as to declare, that it renders theſe ſubſtances ſoluble in water by attracting their air from them, as they ſay of calcareous earth converted into quick-lime; but then they will find themſelves neceſſitated to pronounce, that ſpirit of wine diſſolves the reſins by attracting the air which they contain; which, according to Mr. Crans, would throw them into a labyrinth of difficulties, if not of abſurdities.

Besides, adds Mr. Crans, if it were the abſence of air which conſtitutes cauſticity, it would follow, that all the neutral ſalts muſt be cauſtic, as the air has been expelled in their combination, by the efferveſcence; we, however, find, that they are more mild, than either of the ingredients, of which they are compoſed, was in its ſeparate ſtate.

Mr. Crans then proceeds to the diſſolution both

both of lime-ſtone and chalk in acids. He obſerves, that we may have an efferveſcence or not, in theſe operations, as we pleaſe. The efferveſcence is very briſk if we employ an acid moderately concentrated; and there is none, if the ſame acid be diluted in a great quantity of water. However, ſays Mr. Crans, if fixed air be one of the conſtituent principles of calcareous earths and ſtones, why does it not diſengage itſelf under the laſt circumſtance? And if it be diſengaged, what becomes of it, ſince it does not announce its ſeparation by an efferveſcence?

Mr. Crans ſhews afterwards, that we may obtain a briſk efferveſcence, by mixing together the cauſtic lixivium and an acid; although, according to Meſſrs. Black and Jacquin, it contains no air. His method is to pour, gently, ſome cauſtic lixivium into a ſolution of calcareous earth, the alkali trickles down the ſides of the bottle, and reaches the bottom: if we afterwards agitate the two liquors ſuddenly, to mix them together, a briſk efferveſcence enſues, and the precipitation is formed in an inſtant.

Messrs. Black and Jacquin had declared that quick lime might be made, in the humid way, by precipitating calcareous earth, diſſolved in the nitrous acid, by means of a cauſtic alkali; in fact, according to their doctrine, the calcareous earth not meeting, in the proceſs, with any ſubſtance which can ſupply it with air, muſt remain in the ſtate of quick lime. Mr. Crans denies theſe experiments, and oppoſes them with ſome of a contrary kind: he tells us, that, in whatever way he operated, the calcareous earth precipitated from a ſolution of it in the nitrous acid, whether he employed a mild or a cauſtic fixed alkali, afforded no difference; that in every caſe it effervеſced with acids, and was but a common calcareous earth, except that it had ſome degree of ſolubility in water, and turned ſyrup of violets green. He tried to diſſolve lime itſelf in the nitrous acid, and to precipitate it with the cauſtic alkali; and, notwithſtanding Dr. Black declares, that there is nothing in this combination which can ſupply the lime with air, he, nevertheleſs, obtained a true calcareous earth, which effervеſced with acids.

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ANOTHER kind of proof, of which Dr. Black and his followers avail themſelves, is the precipitation of lime-water by air ſeparated either from an effervefcing or fermenting mixture; but Mr. Crans pretends, that there is no abſolute proof that the precipitation is made by the air; that there are other cauſes which may produce ſimilar effects, and ſuppoſing the air to act in no other manner on the water than to render it more rare, that circumſtance alone would be ſufficient to cauſe the precipitation. Beſides, adds Mr. Crans, how can we conceive that the air, which in the aërial waters is the ſolvent of iron, ſhould here have a quite contrary property, of rendering the lime inſoluble in water*.

MR. CRANS then takes notice of the arguments which the partiſans of fixed air draw from the loſs of weight which calcareous earth ſuffers when diſſolved in acids. Dr. Black and Mr. Jacquin have advanced that when lime-ſtone was diſſolved in an acid, a diminution of weight was

 perceived

* MR. CRANS might add that the aerial waters even diſſolve calcareous earth.

perceived equal to what would have taken place if the same stone had been reduced to lime by calcination; that in both cases the fixed air contained in the lime-stone escaped, in the first case by the effervescence, and in the second because it was driven off by the force of the fire.

Mr. Crans opposes again, here, experiment to experiment; he dissolved several kinds of calcareous stones in the nitrous acid; he also made a solution of lime, keeping an exact account of the weights of the acid and of the earths to be dissolved in it. He commonly observed, in these processes, that there was a sufficiently remarkable diminution of weight, but without any rule; sometimes the lime appeared more diminished than the calcareous earth; at other times the calcareous earth appeared to receive some augmentation of weight in its dissolution. All these results are directly contrary to Dr. Black's doctrine. But it may be objected against Mr. Crans, that he made use of vessels which were too shallow in these last experiments, and more especially that he operated on such small quantities, that an error in the scales might occasion the

the greater part of the inequalities which he has remarked.

After ſome other objections, which I ſhall omit to relate, Mr. Crans proceeded to the decompoſition of ſal ammoniac by lime. He firſt obſerves, that if, according to Dr. Black's hypotheſis, the fire drives from the lime-ſtone, during its calcination, the fixed air with which it was ſaturated, it is impoſſible for the lime, in the decompoſition of the ſal ammoniac, which is made in a retort and in a conſiderable degree of heat, to attract to itſelf the air of the volatile alkali, and he pretends, that ſo far ſhould the lime be from abſorbing it under ſuch circumſtances, that it ſhould rather undergo a new calcination, and loſe that which might ſtill adhere to it; but admitting alſo Dr. Black's hypotheſis, the lime, ſays Mr. Crans, ought to ceaſe to be lime after the operation; he aſſures us, however, that the reſiduum after the decompoſition of ſal ammoniac by lime, uniformly afforded him a calcareous earth in the ſtate of lime, and conſequently deprived of its air; from whence he concludes that it has not attracted the air which was contained in the volatile alkali, and therefore it is not the abfence of air which caufes its caufticity. Laftly, he afferts that fal ammoniac contains much air; that this air, according to Dr. Black, fhould ferve to faturate the lime, and therefore it muft be incapable of any further action on the volatile alkali.

MR. CRANS adds to thefe experiments, that if the action of cauftics really depends on their abforption of air, animals ought to be cauterifed whenever they are placed in an air pump; an infant ought to cauterife its mother's nipples, &c. as in each of thefe cafes there is a privation of air.

MR. CRANS goes on to relate a numerous train of experiments made with Dr. Macbride's apparatus; the reader may recollect that it confifts of two bottles which have a communication by means of a glafs fyphon; into the one we put either fome fubftance capable of fermentation, or a mixture of an effervefcent kind; in the other is to be placed the liquor or whatever matter is to be expofed to the action of the fixed ed air which is feparated. Mr. Crans, fucceffively raifed an effervefcence, with the vitriolic and with the nitrous acid and fixed alkali, in one of thefe bottles, and he procured from lime-water, *placed in the other*, a precipitation, fuch as Dr. Black and Mr. Jacquin mention. He produced the fame effect with air which had ferved the purpofes of refpiration.

MR. CRANS fubmitted, to the fame apparatus, the cauftic Lixivium made after Mr. Meyer's method; the air detached from an effervefcing mixture precipitated from it a white fediment which collected at the bottom of the bottle; the liquor alfo acquired, after a certain time, the property of effervefcing with acids, but he has alfo obferved, that when expofed to the open air, it recovered that property nearly as foon: that it alfo recovered it much more fpeedily, if it were placed over a moderate fire; and that it was at the inftant when the fumes began to arife that it regained the property of effervefcing; hence Mr. Crans concluded that it acquires this property only in proportion as the

cauftic

cauſtic principle or *acidum pingue*, to which it was united, evaporates.

Mr. Crans obſerved the ſame thing in reſpect to the cauſtic volatile alkali detached from ſal ammoniac. He placed one portion of it in a ſtove, another on hot cinders, and he ſubmitted the third to Dr. Macbride's apparatus. At the end of eight hours, all the three effervefced; becauſe, ſays Mr. Crans, of the evaporation of the *acidum pingue*: Dr. Macbrides's apparatus therefore, according to him, operated only in theſe experiments, in a manner which might very naturally have been effected in the open air.

Mr. Crans has extended his inquiries further, and has made a great number of experiments in the ſame apparatus, keeping the veſſels cloſe, and obſerving the weight of the matters employed both before and after the operations. He had always a conſiderable loſs of weight in the bottle which contained the mixture deſigned for effervefcence; he always obtained, on the contrary, an augmentation of weight, of ſome grains, in the other bottle.

Mr.

Mr. Meyer's cauſtic lixivium, ſubmitted to this trial, acquired an augmentation in weight of ten grains.

A Solution of ſalt of tartar obtained five grains.

Spirit of hartſhorn acquired nearly twenty-two grains.

Common ſpirit of ſal ammoniac, three grains.

The cauſtic volatile alkali, twenty grains.

Mr. Crans repeated the ſame experiments, leaving the recipient bottle open, whereas in the former experiments it had been accurately luted.

The ſalt of tartar thus expoſed received an augmentation of five grains in weight, and a ſmall quantity of the ſalt concreted at the bottom of the veſſel.

Mr. Meyer's cauſtic lixivium, on the con-

trary,

trary, loſt two grains in three hours, and depoſited a ſediment.

The liquor, afterwards, and the ſediment at the bottom, effervefced with acids.

The common volatile alkali loſt ſomething of its weight.

The cauſtic volatile alkali, on the other hand, acquired ſome grains, it was no longer cauſtic, but quite mild, and effervefced.

These augmentations in the weights, obſerved in moſt of the experiments made with the cauſtic alkalis, and, in general, almoſt all the experiments made in Dr. Macbride's apparatus, ſeem to furniſh very ſtrong arguments in favour of Dr. Black's opinion. Mr. Crans, however, is not at all embaraſſed in forming an anſwer: he agrees, readily, that the fixed air combines with the liquors placed in the receiving bottle, and that to this cauſe is owing the augmentation of weight which they receive; but he adds, that theſe liquors are impregnated in the ſame manner

ner as common water; he denies, that there is a real combination, or that to ſuch a combination is owing the rendering mild the cauſtic ſalts, and he perſiſts in the belief, that ſuch changes depend on the evaporation of the *cauſtic*, or *acidum pingue*, which neutralized the alkali.

SUCH are moſt of the principal arguments, which Mr. Crans's work contains, againſt Dr. Black's doctrine. I have uſed my utmoſt endeavours to deliver them with all their force. It had, perhaps, been deſirable, that the author had brought them into leſs compaſs; that he had made a ſelection of his experiments; and, eſpecially, that he had not entered into perſonalities againſt Mr. Jacquin, which are entirely foreign to his ſubject.

CHAP-

CHAPTER XIV.

THE OPINION OF MR. DE SMETH OF THE ELASTIC VAPOURS WHICH ARE SEPARATED FROM BODIES, AND OF THE PHENOMENA OF LIME, AND THE CAUSTIC ALKALIS.

WHILE Mr. Crans attacked Dr. Black's doctrine of fixed air in the calcareous earths and in alkalis; while he ſhook the foundations on which this doctrine was eſtabliſhed, two learned gentlemen, Mr. de Smeth at Utrecht, and Dr. Prieſtley at London, were each employed on their parts in throwing light on this matter by new experiments. They publiſhed, nearly at the ſame time, two diſſertations, replete with intereſting facts and important diſcoveries. Although Dr. Prieſtley's experiments, having been read at the meetings of the Royal Society of London, ſome months before the publica-

publication of Mr. de Smeth's work, have acquired, thereby, a confiderable priority of date, yet as Dr. Prieftley has further exceeded the bounds of our knowledge on this fubject, and as it is to him we owe fome facts which feem to difcover a new order of things, the natural arrangement of ideas obliges me firft to give an account of Mr. de Smeth's experiments, and I fhall terminate this hiftorical effay with thofe of Dr. Prieftley.

Mr. de Smeth's differtation is written in Latin, and in form of a thefis; he printed it at Utrecht in the Month of October 1772, under the title of a differtation on fixed air. Small 4to. 101. pages.

Mr. de Smeth, firft afferts, that we have no knowledge of the common air which compofes our atmofphere, except by fome phyfical effects; but that we have no idea of its nature, compofition, and chemical combination; from whence he concludes that it is contrary to the principle of found philofophy, to affirm that any fubftance is air, becaufe it affords us

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one or two properties which it may poſſeſs in common with air; that all thoſe who have ſpoken of the elaſtic vapours, ſeparated from bodies whether by fermentation, by fire, or by the effervefcence produced by the mixture of acid with alkaline ſubſtances, have fallen into this error, that they have only conſidered the ſubtlety, the elaſticity, the ſpecific gravity of theſe vapours; but they ſeem to have forgotton and diſregarded many other properties which are not leſs eſſential to air; that by this method of philoſophiſing, water reduced into vapours, ought alſo be denominated air, that we ſhould give the ſame name to the elaſtic fluid, and to an infinite number of incoercible vapours which have no property of air but its elaſticity and its ſubtlety; in fine, Mr. de Smeth goes ſo far as to ſay that elaſticity is a very equivocal characteriſtic of air; that we may ſay the ſame of each particular property of it with which we are acquainted, and he undertakes to prove this in the courſe of his work.

After having ſhewn by experiments, already known, that air is a true ſolvent, in the chemical

cal acceptation of that term; that it diffolves water and vapours, in the fame manner that water diffolves falts, and that it keeps thefe bodies fufpended, contrary to the laws of hydroftatics, Mr. de Smeth proceeds to fome experiments on the effect of air on fome bodies, which if they be not entirely new, are at leaft but little known.

Mr. Szathmar had demonftrated in 1771, in a differtation on Homberg's pirophorus, or phofphorus, that this fubftance increafed fenfibly in weight even during the time in which it fumed, grew hot, and took fire; Mr. de Smeth has examined the circumftances of this phenomenon, in conjunction with Mr. Hann, Profeffor in Medicine, in the Univerfity of Utrecht, and the following is the refult of their experiments.

On the 22d. of November 1771, Mr. Hann placed 272 grains of pirophorus in an exact and fenfible balance; the pirophorus took fire immediately; and in half an hour, its weight received an increafe of twenty grains; the next day it was increafed to twenty-one grains; feven days after it had acquired fifteen grains more, and the whole augmentation was then nearly a fifth, after which it had no ſenſible increaſe except what depended on the variations in the cold, heat, and moiſture of the atmoſphere.

Two hundred grains of pirophorus which had been kept a long time, and which had loſt the power of ſpontaneous inflammability, having been ſubmitted to this trial, at the end of three days, had augmented one-tenth of its weight: Mr. Smeth obſerves that the augmentation was not greater in this experiment, becauſe not having been inflamed, it had undergone leſs heat, and conſequently fewer of its parts had been diſſipated and reduced to vapour.

These obſervations on the increaſe in the weight of the pirophorus, led Mr. de Smeth to that which takes place in quick lime: twelve ounces of this ſubſtance expoſed to the air in a balance, augmented, almoſt viſibly, in weight, during the firſt month: after this period, its attractive power diminiſhed inſenſibly, and at the end of a year or thirteen months, it was abſo-

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lutely loſt. The lime, in this time, had acquired an augmentation in weight of four ounces, three drachms and forty grains, was reduced to a fine powder, and no longer ſeparated the volatile alkali from ſal ammoniac, but in a concrete form.

The whole weight then of this lime, after a ſpace of three months, was ſixteen ounces, three drachms and forty grains; Mr. de Smeth then weighed ſeparately twelve ounces, three drachms and forty grains. After which he made the following calculation; if ſixteen ounces, three drachms and forty grains of lime ſlaked by the air, contain four ounces, three drachms, forty grains of matter drawn from the atmoſphere, how much ſhould twelve ounces, three drachms and forty grains contain? He found that the quantity ſhould be three ounces, two drachms, fifty four grains and half. It was natural to believe that this matter thus attracted from the atmoſphere would eaſily be diſſipated by fire; to aſſure himſelf of this, he put theſe twelve ounces, three drachms and forty grains of lime into an earthen retort, ſuch as is commonly made uſe of for the diſtillation of phoſphorus, and he expoſed it to a very ſtrong fire during two hours: during the operation, there paſſed over into the receiver, one ounce, four drachms, forty grains of pure phlegm, in which not the leaſt veſtige of any ſaline matter could be diſcovered, by any kind of trial. However cloſely Mr. Smeth attended, he could not perceive, during the whole time of the proceſs, any ſeparation of elaſtic matter; but as after the fire was extinguiſhed the retort was found to be cracked, no certain inference is to be drawn from this experiment. The lime, when taken out of the retort, weighed ten ounces, five drachms; which added to one ounce, four drachms, forty grains of phlegm, amounts in the whole to twelve ounces, one drachm, forty grains; whence it follows that the loſs of weight during the calcination was only two drachms. It is evident then that if there had been a detachment of air, during the diſtillation, it was by no means ſo conſiderable as it ſhould have been according to Dr. Black's ſyſtem; for we may recollect, in fact, that, from his account, it ſhould be nearly one half of the weight of the calcareous earth employed. Mr. de Smeth aſſures us, further,

ther, that the residuum in the retort was a true quick lime.

This experiment gave Mr. de Smeth room to remark that lime slaked in the open air and afterwards calcined in close vessels, does not lose again the whole which it had attracted from the atmosphere. It has been seen, in fact, that the slaked lime contained, before it was submitted to distillation, three ounces, two drachms, fifty-four grains and a half of matter attracted from the atmosphere; it lost, by distillation, no more than one ounce, seven drachms, forty grains; there remained then one ounce, three drachms, fourteen grains which the degree of fire employed was not able to separate. Mr. Duhamel had observed the same thing in a Memoir on Lime, read before the Academy of Sciences in 1747, and which may be found among the papers of that year; I shall presently give an account of his experiments; I have only deferred it hitherto, that the thread of the history of fixed air might meet with no interruption.

This singular circumstance engaged Mr. de

Smeth to repeat this experiment in open veſſels; for this purpoſe, he placed in a crucible the remaining four ounces of the ſame lime which had been flaked by the air. It ought to have contained, according to the above proportions, eight drachms, forty-ſeven grains of matter attracted from the atmoſphere: however, the lime having been urged by a very ſtrong fire, in a wind furnace, loſt no more than ſeven drachms, thirty-ſix grains; and, therefore, had retained one drachm, eleven grains of the matter which had been attracted. The ſame lime, being again expoſed to the air, regained an increaſe of weight, of four drachms, twenty-eight grains.

Mr. de Smeth concludes from theſe experiments: 1ſt, That lime attracts, from the atmoſphere, a ſubſtance which it is impoſſible to deprive it of again. 2dly, That the principal increaſe in weight which it acquires by expoſure to the air, is owing to the water only, and that the air does not ſenſibly contribute to this increaſe by the combination of its proper ſubſtance. He thinks, with Mr. Szathmar, that it is the ſame caſe with regard to the augmentation of weight in in the pirophorus, and that this is equally owing to humidity alone. It is obvious, that theſe aſſertions are directly contrary to thoſe of Doctor Black and his diſciples.

After ſome reflections on the manner in which air exiſts in water, and on the cauſe of ebullition in the latter, Mr. de Smeth undertakes to prove, that if the cauſtic alkalis do not efferveſce with acids, it is probably not to the deficiency of air, or elaſtic matter, that we ſhould attribute this phenomenon; and the following is his manner of reaſoning:

" Dr. Black, and the partiſans of fixed air, " ſuppoſe, that cauſtic alkalis no longer effer- " veſce with acids, becauſe the lime, which has " a ſtrong affinity with fixed air, has deprived " them of that which they contained. If this " principle were true, two things muſt neceſſari- " ly follow: 1ſt, That the cauſtic alkalis ought to " be entirely free from the matter proper for " efferveſcence or ebullition. 2dly, That on re- " ſtoring to them a ſufficient quantity of air, " they ought inſtantly to recover their property

" of effervefcing; but experience, adds Mr. de " Smeth, demonftrates, that both thefe confe- " quences of Dr. Black's theory are equally " falfe." And this is what he undertakes to prove by the following experiments:

EXPERIMENT I.

He placed fome volatile fpirit of fal ammoniac, prepared with lime, under the receiver of an air pump. To the apparatus was affixed an accurate barometer, conftructed in fuch a manner, that the mercury rofe, at each ftroke of the pifton, inftead of falling as in the air pumps ufed in France; as foon as the mercury had rifen to twenty-five inches, the volatile fpirit began to boil brifkly.

EXPERIMENT II.

Having repeated the fame experiment with the common volatile alkali drawn from fal ammoniac

moniac with fixed alkali, and having made a much more perfect vacuum, only a few bubbles appeared, which were ſcarcely perceptible.

EXPERIMENT III.

SOME ſoap-ley was placed under the ſame receiver. As ſoon as the mercury arrived at nineteen inches it began to yeild ſome bubbles: theſe bubbles inſenſibly acquired the appearance of pearls; they did not, however, burſt at the ſurface, but when the mercury was riſen to the height of 28 ¾ inches, they became much larger, and arrived at the ſurface, but without lifting it up: there were many which remainedattached to the interior ſides of the veſſel.

EXPERIMENT IV.

THE common alkalis, how long ſoever they were kept in the vacuum, did not ſuffer the leaſt bubble of air to eſcape, unleſs they had been ſtrongly heated.

Mr. de Smeth concludes from theſe experiments, that the cauſtic alkalis are more diſpoſed to ebullition than the common alkalis; but it is very apparent, that he ſuppoſes the property of effervеſcence to depend on the ſame principle which occaſions liquors to boil, which is not proved: I ſhall have occaſion to recur ſome time to this article.

Mr. de Smeth endeavours to prove, next, that the introduction of air into cauſtic alkalis, does not reſtore to them the property of effervеſcing with acids. To prove this, he procured two bent tubes of glaſs to be ſoldered to the large bulb of a thermometer; he filled the bulb with volatile cauſtic alkali, and he blew through one of the tubes in ſuch a manner as to make the air bubble into the liquor; but although he continued this trial a long time, the alkali had not acquired the property of effervеſcing.

He tried to keep the fixed and volatile cauſtic alkalis in the machine for condenſing air, deſcribed

ſcribed in Graveſande's philoſophy, and he did not obſerve that they underwent any change*.

Mr. de Smeth concludes, from theſe experiments, that the non-efferveſcent property of the cauſtic alkalis proceeds rather from a ſubſtance added to them, than from any thing taken from them; unleſs, adds he, that the lime takes one thing from them and gives them another, which he thinks is a matter difficult to decide upon.

Mr. de Smeth has alſo repeated moſt of Dr. Macbride's experiments, on the effect which the vapours ariſing from fermenting or efferveſcing ſubſtances produce on lime water and the cauſtic alkalis; but inſtead of Dr. Macbride's apparatus he ſubſtituted a ſimple glaſs cucurbit to which a tubulated head was adapted; at the bottom of the cucurbit he placed ſome chalk or alkaline ſalts; on which he poured through the tube, by means of a funnel, ſome kind of acid,

* It appears, that Mr. de Smeth ſuppoſes here, that the elaſtic fluid, which affords the power of efferveſcence to the fixed and volatile alkalis, is the ſame with that we breathe, which is contrary to his own opinion, as we ſhall preſently ſee.

and immediately closed the tube, and then fastened to the extremity of the beak of the head a phial which contained the lime water, caustic alkali or other matter which he chose to expose to the vapours arising from the fermenting or effervescing mixtures.

The caustic volatile alkali, exposed in this apparatus to the vapours arising from an effervescence occasioned by a solution of fixed alkali, either in the vitriolic, nitrous or marine acid, acquired in all the three cases the property of effervescing, and recovered a concrete form.

Fixed caustic alkali became effervescent in the same apparatus, but did not crystallize.

The acid of vinegar combined with the different absorbent earths produced the same effects.

Quick-lime being substituted to calcareous earths, its combination with acids did not render the caustic alkalis effervescent, nor capable of crystallizing.

Mr.

Mr. de Smeth repeated the ſame experiments with ſugar and water which he had placed to ferment in the ſame cucurbit; at another time he made uſe of rye flower diluted with a certain quantity of water; the vapour which ſeparated while the fermentation was ſtrong, produced preciſely the ſame effects as that from effervеſcing mixtures.

Every time the cauſtic volatile alkali was ſubmitted to this trial, there always formed at the top of the bottle which contained it, concretions of volatile alkali in different forms, and reſembling vegetation. One might ſee theſe concretions appear in the liquor; and if the fermentation were briſk, the operation was finiſhed, and the volatile alkali rendered mild in two or three hours.

Mr. de Smeth has further obſerved that in this experiment, a ſmall cloud always aroſe from the cauſtic volatile alkali which directed itſelf towards the beak of the alembic; that at the ſame time an inteſtine motion was obſervable in the liquor, nearly proportionable to the thick-

neſs

neſs of the cloud, and which ſeemed directed upwards. The cryſtals of volatile alkali, obtained in theſe different operations, dried eaſily on filtering paper, and their penetrating odour was nearly diſſipated.

When the fermentation is finiſhed, the elaſtic vapour can ſtill render cauſtic alkalis capable of effervеſcing, but it has no longer the power of making them cryſtallize.

Lime-water, expoſed to the ſame trials, became turbid, and the lime, which it contained, precipitated.

Mr. de Smeth ſuffered fleſh to putrefy in the ſame apparatus, and the vapour which proceeded from it precipitated lime, and reſtored to the cauſtic alkalis the property of effervеſcing; only the effects were produced more ſlowly. As to their property of occaſioning the ſalts to cryſtallize, he could not poſſibly form any judgment of it, becauſe the animal ſubſtances when in fermentation threw off moiſt vapours, which would have diſſolved the ſalt, even ſuppoſing that it had been diſpoſed to cryſtallize.

Mr.

Mr. de Smeth propofes to prove, in the next place, that the elaftic vapours, which arife from either fermenting or effervefcing fubftances, differ effentially from atmofpheric air. I fhall relate, in few words, the principal diftinctions which, according to him, characterife thefe emanations.

1ft, The vapour from effervefcing, or fermenting bodies, reftores to cauftic alkalis the property of effervefcing with acids, and enables volatile alkalis to cryftallize; but atmofpheric air, under the fame circumftances, does not produce the fame effects.

2dly, Atmospheric air fupports, nourifhes, and excites fire; it is even fo effential to the formation of flame, that the latter cannot exift without it: on the contrary, the air from effervefcence, or fermentation, is hoftile to flame, and immediately extinguifhes it. Mr. de Smeth was affured of this fact by a great number of experiments; befides, it is well known to all thofe who make wine, that candles are inftantly extinguifhed in cellars where that liquor is fermenting, if there be not a fufficient renovation of air.

3dly,

3dly, Atmospheric air is equally neceſſary to the ſupport of *animal* life; that from fermentation, on the contrary, is ſo noxious to animals, that, like a ſubtle poiſon, it is fatal to thoſe who breathe it in ſufficient abundance; and it is from this cauſe, that frequent accidents happen in cellars, when they have been ſhut up too ſoon after the vintage; alſo that care is taken not to enter them without precaution, and even not without firſt putting down a lighted candle into them.

The air, ariſing from effervefcence, is not leſs fatal to animals than that from fermentation; it differs, however, from the latter, in not occaſioning intoxication, and in not communicating to bodies the ſame vigour, when taken in ſmall doſes.

4thly, The air of the atmoſphere rather favours than reſiſts putrefaction; whereas, the vapour from fermenting or from effervefcing ſubſtances is a powerful antiſeptic, as Boyle firſt obſerved; as Mr. Cotes has remarked in his lectures, and as Dr. Macbride has ſince confirmed by ſeveral experiments.

5thly,

5thly, The vapour from fermentation* is, ſometimes, wonderfully elaſtic, but that elaſticity is not permanent. At firſt it is very conſiderable; it afterwards becomes more languid, till at laſt it is entirely loſt. The caſe is nearly the ſame with the vapour from effervescence. Though the occaſion of theſe differences be not well known, it may however be compared to the caſe of water, which, when reduced to vapour, is rarified by heat to a great degree, and affords appearances ſimilar to thoſe of air; but when cooled and condenſed, is reduced to a ſimple drop of water.

6thly, The vapour of fermentation is much more ſubtle than common air; it paſſes through bodies which would be an impenetrable obſtacle to the latter. Mr. de Smeth was not able to retain it by the aid of lutes; a moiſtened bladder, tied over the mouth of a veſſel, which contained ſome fermenting matter, was not at all inflated during the height of the fermentation; although it was certain, from other experiments, that a great quantity of elaſtic fluid was diſcharged.

From all theſe experiments, and the reflexions accompanying them, Mr. de Smeth concludes, that the appellation of *fixed air* is very improperly given to the vapours ariſing from fermentation and from effervefcence; that this ſubſtance was long ſince known to Van Helmont by the name of Gas, to Boyle by the name of Factitious Air, and by that of *Æſtus* to the ancients. That it is this which was meant to be deſcribed by the dangerous air of Avernus, by the peſtiferous blaſt of the furies; and that to this we are to aſcribe the cauſe of the fatal effects of the Grotto del Cani, and ſome other ſubterraneous places.

Lastly, Mr. de Smeth concludes, that fixed air or gas is not always one and the ſame ſubſtance, but, on the contrary, is very various, multiplied, and different in itſelf. That ſo far from being a particular element, or being *ſimple* in the ſenſe which chemiſts give to that word, it did not exiſt, primitively, in the bodies from which it is ſeparated, but that it is a miaſma formed by the attrition conſequent upon the colliſion of all the ſolid and fluid parts; that it is, therefore, never produced but in caſes where the

bodies

bodies ſuffer violent inteſtine motions, and tumultuous ſhocks, when their parts, ſtriking againſt each other, are altered, broken, attenuated, as in fermentation, effervefcence, combuſtion, &c. Mr. de Smeth thinks, that it ſhould therefore be diſtinguiſhed into

Gas vinificationis,
Gas acetificationis,
Gas ſepticum,
Gas ſalinum ſeu effervefcentiarum,
Gas aquæ et terræ ſeu ſubterraneum.

He aſſigns, however, nothing to authorize theſe diſtinctions, beſides the ſeveral odours, excepting the gas vinificationis, which occaſions particular phenomena in the animal œconomy.

Mr. de Smeth, afterwards, briefly examines into the opinion of thoſe who eſteem fixed air to be the univerſal bond of the elements, the cement of bodies. It may be eaſily apprehended, after what has been declared, that this opinion is not adopted by him. He does not deny that fixed air is an antiſeptic, but yet, according to him, it does not follow from thence, either that fixed air muſt originally have exiſted in bodies from which it has been diſengaged, or that it muſt have contributed to the coheſion of their parts, or their ſtate of ſalubrity; on the contrary, he obſerves, that this antiſeptic virtue is not particular to fixed air; but that all the products of fermentation poſſeſs the ſame property; that tartar, vinegar, and ſpirit of wine, are equally antiſeptic with fixed air. Laſtly, he adds, that every thing which Dr. Black's diſciples advance, with reſpect to fixed air, is equally applicable to ſpirit of wine; and that we may, by the ſame arguments, maintain that *it* is the cement of bodies and the bond of the elements, which would, notwithſtanding, be abſurd.

Dr. Macbride had advanced a new argument in favour of fixed air, from the manner in which aſtringents act: their antiſeptic virtue, according to his account, depends on the property which they poſſeſs of contracting the pores of bodies, when putrefying, and by that means preventing the ſeparation of the fixed air which

is

is ready to escape. Mr. de Smeth refutes this argument, and asserts, that we know too little of the manner in which astringents act, to be able to form the least induction from thence *.

FROM the whole of this work, Mr. de Smeth concludes, that the doctrine of fixed air is erected

* THE following experiments, which were made by the translator, induced him to suppose, that the sweetening properties of fixed air may possibly depend on an affinity between fixed air and the septic particles arising from putrid bodies, and that this air may act as a *menstruum* on the effluvia emitted by it. A piece of putrid beef, fastened, by a string, to a cork, was confined in three pints of fixed air for thirteen hours, and was, thereby, considerably, though not entirely, sweetened. *But the air in the bottle seemed to have acquired all the putrid smell of which the flesh had been deprived.* So that the septic effluvium did not appear to be destroyed, but to have changed place. Slips of linen cloth, also, dipped in very rancid oil, had their rancidity much diminished by exposure to a stream of fixed air from an effervescent mixture. But a pint bottle of the same oil being saturated with this vapour, was not at all sweetened, though it absorbed much air. *Henry's Experiments and Observations*, page 127 and seq. T. H.

ed upon uncertain and weak foundations; that from the manner in which it is delivered by its favourers, it cannot bear a ſerious examination; and that it will prove to be the mere opinion of a moment.

To this examination of Dr. Black's ſyſtem, Mr. de Smeth adds two intereſting obſervations, on the air of the wells at Utrecht, and on that which proceeds from burning charcoal.

The wells at Utrecht are from eight to twenty feet in depth; it has been the cuſtom to make uſe of pumps to raiſe the water, and they are then covered over with a kind of arch. When, after a certain period of time, the wells are opened, on any account, it is neceſſary to leave them uncovered for twelve hours, before any perſon deſcend into them; whoever ſhould venture to go down into them ſooner, would expoſe himſelf to immediate death. The air of theſe wells extinguiſhes candles, like that acquired from fermentation or effervefcence; it precipitates, alſo, the lime from lime-water, and changes it to a calcareous earth; in ſhort, it has all the proper-

ties

ties of that which we call fixed air; the water, however, from theſe wells, is not the leſs ſalubrious.

Mr. de Smeth has alſo proved, that the air which has paſſed through burning charcoal, has much the ſame properties as fixed air; it precipitates lime-water, and reſtores the property of effervefcing with acids to cauſtic alkalis. He ſhews the manner of combining this air with different ſubſtances, in the vacuum of an air pump; and he obſerves, that when the volatile cauſtic alkali is employed, at the inſtant the air from the charcoal enters the receiver, a very conſiderable aggregation of ſmoke is obſervable, ariſing from the volatile alkali.

It is eaſy to perceive, from the account which has been given of Mr. de Smeth's work, that he has endeavoured to embrace an intermediate opinion between thoſe of Dr. Black, and of Mr. Meyer: but that his ſyſtem, at the ſame time, does not always agree with his own experiments. His treatiſe, otherwiſe, is clear, methodical, and well written. His experiments are properly made, and, in general, accurate and true. I ſpeak, at leaſt, of ſuch as I have had occaſion to repeat; and thoſe are the greater part of them.

CHAPTER XV.

Dr. PRIESTLEY's INQUIRIES concerning different kinds of AIR.

NOTHING now remains to complete the object which I propoſed to myſelf in this firſt part, but to give an account of a numerous ſeries of experiments, communicated laſt year to the Royal Society of London by Dr. Prieſtley *. This work may be regarded as the moſt elaborate, and moſt intereſting, of any which has appeared

* These experiments of Dr. Prieſtley, were publiſhed in Engliſh at the latter end of the year 1772. I had already been, ſome time, employed on the ſame ſubject, and I had declared, in a paper depoſited with the Royal Academy of Sciences, on the firſt of November 1772, that a very great quantity of air was ſeparated, during the reduction of metals.

peared ſince that of Dr. Hales, on the fixation and ſeparation of Air. No modern work has ſeemed to me more adapted to evince how many new roads Philoſophy and Chemiſtry ſtill point out to travel over.

Dr. Priestley's work, being, as it were, a train of experiments, not much interrupted by any reaſoning, an aſſemblage of facts, moſtly new, whether conſidered by themſelves, or by the circumſtances which accompany them, it may be imagined that it is little capable of abridgment; I ſhall therefore be obliged to follow him, ſtep by ſtep, in the account which I am going to give of his experiments, and *my* extract will be nearly as long as *his* treatiſe.

SECTION I.

Of FIXED AIR.

Dr. Priestley firſt examines that which is properly called Fixed Air, which is the product of ſpirituous fermentation or of ſome efferveſ-

cence,

cence. The breweries afforded him a plain and easy method of obtaining a great quantity of this air in an almost perfect state of purity *. There is always a body of it, nine inches in depth over the tubs in which the beer is fermented, and as there is a constant fresh supply from the fermenting liquor, it is but little mixed, for that thickness, with the neighbouring air.

This air, according to Dr. Black's experiments, is heavier than atmospheric air, and it is doubtless for this reason, that it continues, in a manner, attached to the surface of the beer without separating from it; it is also on account of its superior gravity, that it may be conveyed from one room to another in an open bottle, provided the mouth of the bottle be kept upwards; the fixed

* Dr. Priestley speaks of this kind of air as sufficiently pure for many purposes, but, from its continually mixing with common air, far from being perfectly pure. Mr. Lavoisier has, in some other instances, expressed himself in stronger terms than those which Dr. Priestley has used, and sometimes mistaken his meaning; I have, in these cases, often taken the liberty of restoring the Doctor's expressions, instead of translating the words of my author. T. H.

fixed air, for ſome moments, not mixing much with that of the atmoſphere. Though this ſuperiority of weight appear to be well eſtabliſhed by theſe experiments, Dr. Prieſtley relates others, alſo, which might almoſt induce us to change our opinion. He informs us that we may place a lighted candle in a receiver filled with atmoſpheric air, and then plunge it, with its mouth upwards into an atmoſphere of fixed air, and yet the candle ſhall continue to burn. The fixed air, in this experiment, does not, then, diſplace the atmoſpheric air, and conſequently is not heavier: if on the contrary the mouth of the jar, inſtead of being upwards, be turned downwards, and eſpecially if a narrow necked veſſel be employed, the two kinds of air will, in time, be perfectly mixed *. Suppoſing that theſe experiments do not prove an exceſs of gravity in atmoſpheric air; we may at leaſt conclude that they are very nearly

* Mr. Lavoisier has miſinterpreted this paſſage; Dr. Prieſtley's words are, "A candle put *under* a large receiver, and immediately plunged very deep below the ſurface of the fixed air, will burn ſome time. But veſſels with the *ſmalleſt orifices*, with their mouths downwards in the fixed air, will, *in time*, have the common air, which they contain, perfectly mixed with it." T. H.

nearly equi-ponderant, and this ſeems to have been confirmed by Dr. Hales's experiments on air ſeparated from tartar, and thoſe of Mr. Bucquet on that produced from effervefcent mixtures.

Dr. Priestley has alſo obſerved that a candle, charcoal, and a piece of red-hot wood were extinguiſhed, as ſoon as they were plunged into the atmoſphere of fixed air, which occupied the ſurface of a tub of fermenting beer: but it is moſt worthy of remark that this air ſeems to retain the ſmoke, which floats upon the ſurface of it without ſeparation; it there forms a bed ſmooth and well defined, above, but ragged, beneath, ſeveral parts hanging down to a conſiderable diſtance within the body of the fixed air. When this fixed air is very ſtrong, the ſmoke of a ſmall quantity of gun-powder fired in it will be wholly retained by it, no part eſcaping into the common air.

Dr. Priestley has alſo obſerved, that the fixed air from fermenting beer, combines eaſily with the vapour of water, as alſo with the ſmoke

of

of rosin, sulphur and other electrical substances; yet by holding the wire of a charged phial among these fumes, he could not make any electrical atmosphere.

A SHORT time before the publication of these papers, Dr. Priestley had published a small pamphlet * on the manner of impregnating water with fixed air, and communicating to it the properties of those acidulous or aërial waters which are so frequently met with in a natural state. His process consists in receiving the air, produced by an effervescence from chalk and oil of vitriol, into a bladder; making it pass from thence, by means of a syphon, into a bottle filled with water and inverted into a vessel in the form of a bason with a little water in it, and then agitating the bottle strongly: the water, by this process, absorbs almost all the air introduced into the bottle, and by repeating the operation, a quantity of air, something more than equal to the bulk of the water may be united to it. Dr. Priestley

* DIRECTIONS for impregnating water with fixed air, &c. London, 1772.

Prieſtley here gives us a ſtill more ſimple method of producing this union, viz. to place an open veſſel, filled with water, in an atmoſphere of fixed air ariſing from fermenting wort; and it becomes, in a ſhort time, ſimilar to the acidulous waters. This combination is accelerated, by pouring the water from one veſſel to another, expoſed to the ſame atmoſphere; in a few minutes, the Doctor has thus given it an appearance hardly diſtinguiſhable from very good Pyrmont, or rather Seltzer water. The ſame effect may alſo be produced by filling a receiver with fixed air at the breweries, and inverting it into a baſon full of water; the water inſenſibly abſorbs and diſſolves the fixed air, and riſes proportionably in the receiver: this is a very commodious method of uniting fixed air with all kinds of liquors, and may ſerve to reſtore briſkneſs to flat wines and ſpirituous liquors which are become vapid.

It appears from Dr. Prieſtley's experiments, that water cannot abſorb the whole of the air ſeparated by effervеſcence or by fermentation; however pure it may be, a portion remains unabſorbed,

abſorbed, in which though burning bodies be extinguiſhed, animals can, however, reſpire.

It has been already ſeen, from Dr. Hales's experiments, that a mixture of iron filings and brimſtone, placed under an inverted glaſs bell, diminiſhed the bulk of air in which it was confined. Dr. Prieſtley has obſerved the ſame diminution to take place when he employed fixed, inſtead of common, air; and that the fixed air, thus diminiſhed in its volume, did not appear to be ſo noxious to animals as before its diminution, nor to differ ſo much from common air, but he remarks that this change may be attributed to his having inadvertently agitated the diminiſhed air in water. Dr. Prieſtley was once led to conclude from hence, that it is phlogiſton which fixed air wants to make it common air*, though

* In the account which Dr. Prieſtley has ſince publiſhed, of further experiments made on different kinds of air, we find him confirmed in the conjecture, that fixed air is capable of forming an union with phlogiſton, and thereby becoming immiſcible with water. This effect he had before produced by means of iron filings and brimſtone, but has ſince had a much more deciſive and elegant proof of it by electricity. Vide Doctor Prieſtley's Experiments and Obſervations on different kinds of air, page 248. T. H.

though he acknowledged himſelf ignorant of the method of combining them, and that when he calcined a quantity of lead in fixed air, it did not ſeem to have been leſs ſoluble in water than it was before.

Dr. Priestley has alſo repeated moſt of the Honourable Mr. Cavendiſh's experiments on the diſſolving power of water impregnated with fixed air. He has obſerved, with that gentleman *, that it readily diſſolves iron, but that it does not make a complete ſolution of ſoap; that it changes the blue juice of tournſole into red. This laſt obſervation ſeems declarative of its containing ſome portion of acid; ſome experiments, however, will appear, in the ſequel, contradictory to this opinion †. Water, thus impregnated with fixed

* "Or rather with Mr. Lane. It appears, however, "that fixed air, without water, has no ſuch power." Ibid. page 250.

† Mr. Hey's experiments, to which Mr. Lavoiſier here alludes, tend only to prove that water impregnated with fixed air is not made acid *by the vitriolic acid being volatilized and mixed with it.*

fixed air, eaſily parts with it when heated, when freezing, and in the vacuum of an air pump.

Dr. Priestley was deſirous of knowing, from his own obſervation, the effect of fixed air on animals: thoſe which breathed it, periſhed inſtantly; he remarked, that their lungs were white and collapſed, and he could not perceive in them any other cauſe of death. Inſects, ſuch as butterflies, and flies of other kinds, ſoon become torpid and apparently dead, but may eaſily be revived, by being expoſed to a current of common air. The effect is nearly the ſame on frogs; but a ſnail, treated in the ſame manner, died preſently.

Fixed air is not leſs fatal to vegetable than to animal life. A ſprig of mint, growing in water, placed over ſome fermenting liquor, became quite dead in one day; a red roſe became of a purple colour, on being expoſed to the vapour from fermentation, in twenty-four hours; but various other flowers were little affected *.

When

* Dr. Priestley relates that another roſe turned perfectly white in the ſame ſituation; but he acknowledges

that

When Dr. Prieſtley had ſeparated fixed air from chalk by its combination with acids, he tried alſo to detach it by fire; and, for this purpoſe, made uſe of a gun-barrel. One half of the air which he obtained, by this proceſs, was capable of being abſorbed by water, the other half inflammable.

SECTION II.

Of Air in which a Candle, or Brimstone, has burned out.

AFTER having examined the properties of fixed air, Dr. Prieſtley relates the experiments he

that the experiments were not repeated, and expreſſes a wiſh that it might be done, in pure fixed air extracted from chalk by means of oil of vitriol. In the ſummer of 1774 the tranſlator of this treatiſe, expoſed ſeveral roſes, and other flowers to the vapour ariſing from an effervefcing mixture, formed with thoſe ingredients, without perceiving any change in their colour; nor does fixed air ſo obtained prove fatal to vegetation, as appears from ſome ingenious experiments with which Dr. Percival intends to favour the public. T. H.

he has made on portions of atmofpheric air, which he confined in glafs receivers, and in which candles or fulphur had burned out.

The air, thus confined, diminifhed about one-fifteenth, or one-fixteenth, in its bulk, which is one-third as much, according to Dr. Prieftley, as can be induced, either by the refpiration of animals, the corruption of animal or vegetable fubftances, the calcination of metals, or by a mixture of fulphur and iron filings. One fingular circumftance, and which may throw fome light on this phenomenon, is that this diminution does not, always, immediately take place; but that it is fometimes not reduced till it has paffed feveral times through a quantity of water, and been agitated with it; the fixed part combines with the water, and it is not till then that the diminution is effected. But the diminution is generally inconfiderable, if the air have ftood in quickfilver, there not being any fubftance expofed to the air that could abforb any part of it.

These experiments of Dr. Prieftley confirm

what

what Dr Hales had ſuſpected, viz. that air, confined in a receiver, does not diminiſh in bulk, in proportion to the quantity of ſulphur burned in it. Dr Prieſtley has proved that this diminution has bounds which cannot be exceeded, and that, whenever he uſed a ſufficient quantity of ſulphur, it was always the ſame in proportion to the ſize of the receiver.

Atmospheric air confined in a receiver, acquires the property of uniting with lime-water and precipitating the lime, when either wax candles, tallow candles, ſpirit of wine, ether or any other ſubſtance, except brimſtone, is burned in it*; and the reaſon why the laſt named body does not produce the ſame effect, Dr. Prieſtley thinks, may be deduced from the acid vapour of the ſulphur uniting with the lime, diſſolving it, and preventing its precipitation.

Dr.

* Dr. Priestley ſuſpected, at that time, that the phenomena produced in common air by theſe methods, were cauſed by the action of phlogiſton let looſe from theſe bodies, in the proceſs, overcharging the air, and ſubſequent experiments have tended to confirm him in this opinion,

Dr. Hales, in his vegetable ſtatics, attributes the diminution of the volume of air to the loſs of its elaſticity; in this caſe, the air ſo reduced, ought to have a greater degree of ſpecific gravity than it before poſſeſſed; Dr. Prieſtley, however, believes, that it certainly becomes, on the contrary, rather lighter; and he therefore concludes that it is the fixed part of the air and its heavier portion which is precipitated.

It is univerſally known that a wax or tallow candle, lighted and placed under a receiver, cannot burn long; it goes out; and if we attempt to place freſh ones there, they are immediately extinguiſhed. M. de Saluces, in the Turin Memoirs, vol. I. page 41, attributes this effect to the rarefaction cauſed by the heat, and he ſuppoſes that the air may be recovered by compreſſing

opinion, having found that every body which emits phlogiſton, and among others, the electric ſpark, diminiſhes common air. He conjectures that the phlogiſton having a nearer affinity with ſome of the conſtituent parts of the air, than the fixed air which enters into the compoſition of it, the latter is conſequently precipitated.

preffing it in bladders. Dr. Prieftley agrees with him as to the truth of the experiments, but he denies the confequences: he imagines that the effect cannot be owing to compreffion alone, becaufe the experiment will not fucceed, except in bladders: and he affures us that he had tried, in vain, a very ftrong degree of compreffion, in glafs veffels, without the quantity of the air being reftored. He alfo brings another experiment in aid of the above; he filled an exhaufted receiver with air which had paffed through a glafs tube made red-hot, and found that a candle would burn in it perfectly well. The extinction, therefore of wax and tallow candles, confined in clofe veffels, cannot be owing to the rarefaction of the air only.

Animals, from Dr. Prieftley's experiments, live as long in air in which candles have burned out as in common air. He alfo obferved the fame of air in which brimftone has burned, after the vapours have had time to fubfide. Neither is this air more noxious to vegetables; Dr. Priftley has kept different kinds of plants growing in it, without their being particularly affected ed by it. But the moſt remarkable circumſtance was, that the air was afterwards re-eſtabliſhed in the ſtate of common air, and candles again burned in it as at firſt *.

SECTION III.

Of inflammable air.

Dr. PRIESTLEY firſt deſcribes the method by which he obtains inflammable air, which is the ſame as that deſcribed by Mr. Cavendiſh in the Philoſophical Tranſactions: it conſiſts in forming ſolutions of iron, zinc or tin, but eſpecially the two former, in the vitriolic acid, and catching the air or rather elaſtic fluid, ſeparated by the effervefcence, either in bladders or otherwiſe. But when he extracted it from vegetable or animal ſubſtances, or from pit-coals, he put them into a gun barrel, to the orifice of which he

* Dr. Priestley conjectures that this recovery of the air to it's former ſtate, is effected by the plants imbibing the phlogiſtic matter with which it is overloaded by the burning of inflammable bodies.

he luted a glafs-tube or the ftem of a tobacco-pipe to the other end of which he tied a flaccid bladder.

The quantity of inflammable air which is obtained by the procefs, depends very effentially on the degree of heat employed. A fudden and violent heat procures fix or feven times as much as when the heat is applied more gradually, though increafed to ever fo great a degree, at the end of the operation. A bit of dry oak weighing ten or twelve grains, will generally yield about a fheep's bladder full of inflammable air, fuppofing the heat to be fufficiently brifk.

Dr. Priestley has alfo remarked, in this refpect, that the air obtained from folutions is more inflammable in proportion as the effervefcence is more brifk; but in this experiment, as in all the others, he made ufe of bladders; and it muft be acknowledged, that this circumftance is capable of throwing fome uncertainty on the refults. The doubts which may be formed, on this head, feem alfo confirmed by other paffages in his memoir: he owns that inflammable air penetrates

penetrates bladders, and even cork, and that there is no other way of preserving it, than by corking closely the bottles which contain it, and inverting them with their mouths downwards, into a vessel of water.

Having instructed us how to obtain and preserve inflammable air, Dr. Priestley examines into the degree of its affinity with water; he first remarks, that if it be kept in a bottle inverted into a bason of water, the surface of the water becomes covered with a fixed matter, which is a red okre when it has been generated from iron, but whitish when extracted from zinc.

Though the combination of this air with water, be not any thing near so easy as that of fixed air, it may however be accomplished by strong agitation. About a fourth part of inflammable air is absorbed in this operation; if the agitation be long continued the air loses its inflammability, and the remainder does not seem to differ from common air *.

Inflammable

* Dr. Priestley says, "it admitted a candle to burn in it like common air, only more faintly; and indeed by the

Inflammable air, produced from oak, had this peculiarity, that water abforbed about one half of its bulk; but this circumftance is probably to be accounted for, from the mixture of a portion of *fixed*, with the *inflammable*, air. The refiduum alfo in this, as in the preceding experiment, differs not from common air.

Dr. Priestley did not fail to examine into the effect of inflammable air on animals, and vegetables; the firft were thrown into convulfions, which foon terminated in death †, nearly in the fame manner as when they are plunged into fixed air. Whatever number of animals thus perifhed in it, the noxious quality of the air was not diminifhed, and its action was as great on

the teft of nitrous air, it did nor appear to be near fo good as common air." T. H.

† Two wafps, having been put into inflammable air, and fuffered to remain in it near an hour, prefently ceafed to move and feemed to be quite dead for about half an hour after they were taken into the open air, but then they revived, and, were foon, quite recovered. Prieftley's Experiments and Obfervations on Air, page 247. T. H.

on the laſt as on the firſt. As to vegetables, it did not appear that inflammable air, remarkably, prejudiced their growth; theſe laſt experiments were made with air drawn from zinc and from oak.

These different experiments induced Doctor Prieſtley to imagine, that different kinds of air, mixed together, might correct each other: he, accordingly, proceeded to mix inflammable air, with air which animals had reſpired, and he found that the mixture was not inflammable. The ſame event did not reſult from a mixture of fixed with inflammable air, for the latter retained its inflammability; they even appeared to have had very little influence on each other, for though they were kept, thus mixed, during three years, they were eaſily ſeparated by ſimple agitation in water. All the fixed air was abſorbed, and the remainder was equally inflammable as at firſt.

It was natural to ſuppoſe that inflammable air muſt be loaded with phlogiſton; yet it could not be abſorbed either by oil of vitriol or ſpirit of nitre, notwithſtanding the great affinity which thoſe acids have with phlogiſton; it does not unite

unite with the fumes of ſmoking ſpirit of nitre, nor is its inflammability even diminiſhed *.

SECTION IV.

OF AIR INFECTED WITH ANIMAL RESPIRATION OR PUTREFACTION.

AIR which has ſerved, ſome time, for the reſpiration of animals, has loſt the property of ſupporting the lives of other animals. When an animal has died in this air and another is ſubſtituted to the firſt, it periſhes inſtantly and in the firſt attempt to reſpire. It ſhould ſeem however that animals may, by habit, become capable, to a certain point, of breathing noxious air: Dr.

* DR. PRIESTLEY has ſince diſcovered that the electric ſpark, taken in any kind of oil, produces inflammable air; that inflammable air is not changed by being made to paſs ſeveral times through a red hot iron tube; and that it is no more diminiſhed or changed by the fumes of liver of ſulphur, or by the electric ſpark, than he had before obſerved it to be by a mixture of iron filings and brimſtone. Prieſtley's Experiments and Obſervations on Air, page 247.

Dr. Prieſtley has actually obſerved that although an animal will live tolerably in air in which it has continued for a conſiderable time, yet if another animal be placed in the ſame air, it will periſh immediately, though the former will live for ſeveral minutes longer. Young animals, cæteris paribus, bear this trial longer than old ones. Theſe circumſtances occaſion ſuch frequent variations in the reſult of the experiments, that nothing can be preciſely determined from them, without repeating them very frequently.

Air which has thus ſerved the purpoſe of animal reſpiration is no longer common air; it approaches to the nature of fixed air, in as much as it is capable of combining with lime-water * and precipitating the lime from it, in form of a calcareous earth; but it differs from fixed air, 1ſt. That being mixed with common air it diminiſhes its bulk †, whereas fixed air increaſes it; 2dly.

* The whole of the air which has been reſpired does not combine with lime-water. It is the fixed principle which has been precipitated, by this proceſs, from the air which poſſeſſes this property.

† The phlogiſton emitted by animal reſpiration diminiſhes

2dly. That it can remain in contact with water without being abſorbed by it; 3dly. That inſects and vegetables can live in it, whereas they periſh in fixed air.

Dr. Priestley, afterwards, ſhews, that there is a very perfect analogy between this air, and that in which animal or vegetable ſubſtances have putrefied: both extinguiſh flame, and are fatal to animals; both equally precipitate lime-water; the gravity of each is equal, and each of them may be reſtored to the ſtate of common air by the ſame methods: the Doctor concludes, from this analogy, that the principal uſe of the lungs, in animals, is to procure the evacuation of a putrid effluvium, which would corrupt the living body in the ſame manner that it produces corruption in thoſe which are dead.

Dr. Priestley had the curioſity to inquire into

niſhes common air; but air, ſo diminiſhed, is not capable of producing any diminution in a quantity of freſh air. T. H.

into the diminution of air which is produced either by the putrefaction of animal fubftances, or by the refpiration of animals. A moufe having putrefied in a given quantity of air, its volume was increafed for a few days, but it afterwards diminifhed, and in about eight or ten days, if the weather were warm, the diminution was found to be $\frac{1}{6}$ or $\frac{1}{5}$. Sometimes the diminution does not appear, till after the air has been made to pafs, two or three times, through water. The fame is the cafe with air which animals have breathed; alfo, air in which candles have burned out may, almoft always, be further reduced by this means.

DR. PRIESTLEY has repeated the fame experiments, only making ufe of mercury inftead of water; he remarked an augmentation in the bulk of the air, for the firft days, perhaps about $\frac{1}{20}$. After this it ftood in the quickfilver two days without any fenfible alteration; and then, on admitting water to it, it began to be abforbed, and its quantity was diminifhed about $\frac{1}{6}$. If he made ufe of lime-water in this experiment, it became turbid, and precipitated, which

fhewed

ſhewed that this air was, in part, in the ſtate of fixed air.

Having alſo placed ſome mice in a veſſel, the mouth of which was inverted into quickſilver, Dr. Prieſtley did not perceive, when they were dead, that the air had been much diminiſhed, but having withdrawn the mice, and introduced ſome lime-water into the veſſel, the volume of air became diminiſhed, and the lime was precipitated.

Hitherto Dr. Prieſtley had operated only on common air, corrupted by the effluvia of putrefied animal ſubſtances, or, which is the ſame thing, on a mixture of common air, and air ſeparated by the putrid fermentation. He thought proper to operate on the ſame effluvia, without any mixture of common air, and his experiments afforded him ſome remarkable phenomena. He put dead mice into veſſels full of water, and inverted the mouths of the jars into baſons alſo filled with water; they produced a conſiderable quantity of elaſtic matter which was not abſorbed by water; after a ſhort time the water contracted an extremely fetid and offen-

ſive

five smell, which seems to indicate that the putrid effluvium pervades the water, and affects the neighbouring air *. He repeated this experiment in a jar filled with quicksilver, and he procured a considerable quantity of air which was absorbed by lime-water in the same manner as if it had been fixed air. These two last experiments seem contradictory to the former; it has been seen, in fact, that the putrefaction of animal substances diminished the bulk of common air in which they were confined, we see here, on the contrary, a considerable production of elastic matter.

To reconcile these phenomena, Dr. Priestley persuaded himself that the putrid effluvium is fixed air mixed with some other vapour which has the property of diminishing common air, in proportion as it is combined with it. This conjecture, however, is not confirmed by experiment;

* MR. LAVOISIER makes Dr. Priestley say that water did not absorb this air, whereas he expressly mentions the water being saturated with it. Priestley's Experiments, page 84. T. H.

ment; for having attempted to mix air ſeparated by putrefaction with common air, no common air having been previouſly ſuffered to communicate with the former, he did not remark any diminution in its bulk *.

Again, according to Dr. Prieſtley, we may vary all theſe phenomena, by varying the circumſtances of the experiment: If, for example, a piece of beef or mutton, raw or boiled, be placed under an inverted jar, filled with, and ſtanding in mercury, and ſo near to the fire that the heat to which it is expoſed be at leaſt equal to that of the blood; a conſiderable quantity of air will be generated in a day or two, about $\frac{1}{7}$ of which will be abſorbed by water, while all the reſt will be inflammable. A mouſe, under this circumſtance, and with the ſame degree of heat, furniſhes a putrid vapour which extinguiſhes the flame of wax or tallow candles.

The

* Dr. Priestley tells us that in the way he made the experiment, he was obliged to let the putrid air paſs through a body of water which might inſtantly abſorb the phlogiſtic matter that diminiſhed the common air. Experiments, &c. p. 85. T. H.

The air which is produced from vegetables, under ſimilar circumſtances, is almoſt wholly fixed air, and does not contain any inflammable part. Putrid cabbage, green or boiled, yields products exactly ſimilar in every point, to thoſe which are obtained from animal ſubſtances.

Animal reſpiration, fermentation, combuſtion, in ſhort, effluvia of every kind would infect the air of the atmoſphere, and render it mortal to all animals, if nature had not a method of reducing the corrupted air to the ſtate of common air. This object engaged much of Dr. Prieſtley's attention, and the following are nearly the reſults of his experiments. He firſt proved that ſimple agitation with water was not capable of depriving air, thus infected, of its noxious quality, unleſs the agitation were continued for a very long time, a circumſtance which could not happen in the common order of nature. He then tried to mix this air with that diſcharged from ſalt-petre, by exploſion, and with the fumes of ſulphur; he ſubmitted it to the teſts of heat, rarefaction, and condenſation; but they all proved unſucceſsful. One method only ap-

 peared

peared, to him, to ſucceed, in reſtoring air to a ſalubrious ſtate, and he ſuſpected it to be the method employed by nature; it is the vegetation of plants. With this view, he has made many experiments, from whence it is evinced, that plants, confined in receivers filled with infected air, vegetate in that ſituation, and, after ſome time, the air becomes as proper as that of the atmoſphere for the reſpiration of animals.

Dr. Priestley has demonſtrated that four parts of fixed air, mixed with one of corrupted air, form air proper for reſpiration; but as this mixture is not made without being ſeveral times decanted, in water, from one jar to another, he apprehended that its being thus paſſed might contribute as much, if not more than the mixture of the fixed air, to render the air wholeſome.

Dr. Priestley obſerves further, upon this ſubject, that every ſpecies of noxious air, whether infected by reſpiration or putrefaction; whether proceeding from the vapour of burning charcoal, or having ſerved in the calcination of metals; whether it have contained, for a long time,

time, a mixture of iron filings and brimſtone, or of oil and white lead; may be always reſtored to ſalubrity by agitating them long with water. The volume of air is diminiſhed by this proceſs, when water is uſed which has been deprived of its air: but it is augmented, on the contrary, when well-water is employed which contains much air. This general aſſertion ſeems to contradict what Dr. Prieſtley had advanced, in another place, viz. that agitation with water was not ſufficient to deprive corrupted air of its noxious qualities *.

SECTION V.

Of air in which a mixture of brimstone and iron filings has stood.

FROM Dr. Hales's experiments, we had learned, that a paſte made with powdered brimſtone

* It was not agitation, but merely decantation from one veſſel to another, which had proved inſufficient in other experiments. T. H.

ſtone and iron filings moiſtened with water, occaſions a conſiderable diminution of the volume of air in which it is placed. Dr. Prieſtley has repeated this experiment, in receivers immerſed both in quickſilver and in water: the diminution was equal in both caſes, but he obſerved, that it could not exceed a fourth or a fifth of the whole volume of air contained in the receiver. Air, thus diminiſhed, is lighter than common air, but it does not precipitate lime-water.

Dr. Priestley attributes this laſt circumſtance to the acid vapour, which exhales from the mixture, during the operation, combining with the air, and forming a ſelenitic ſalt with the lime, inſtead of precipitating it. As a proof of this he obſerves, that the water, which has been uſed in the proceſs, has a manifeſt ſmell of volatile ſpirit of vitriol. If, inſtead of making this experiment in common air, it be made in air which has been already diminiſhed, whether by candles burning in it, or by putrefaction, though it never fails to diminiſh it ſomething more, it is, however, no further than this proceſs alone would have done it. Dr. Prieſtley has remarked, ed, that air thus reduced, by a mixture of brimſtone and filings of iron, was very noxious to animals, and he did not perceive that it was rendered more ſalutary by being placed in contact with water *.

SECTION VI.

Of Nitrous Air.

Dr. PRIESTLEY gives the name of nitrous air to the elaſtic fluid which is ſeparated during the diſſolution of iron, copper, braſs, tin, ſilver, quickſilver, biſmuth, or nickel, in the nitrous acid, and of gold, or regulus of antimony, in aqua regia.

This

* Dr. Priestley has given us ſome curious experiments, in the ſecond part of his ingenious treatiſe on air, tending to prove, from the diminution of common air by the electric ſpark, that the latter either is, or contains, phlogiſton, ſince it does the very ſame thing that phlogiſton does. And further proving that the diminution of common air by bodies emitting phlogiſton is owing to the precipitation of it's fixable part. T. H.

This air has a ſtrong diſagreeable ſmell, differing but little from that of ſmoking ſpirit of nitre. It has the ſingular property of becoming turbid when it is mixed with common air, of aſſuming a red or deep orange colour, and of producing a ſtrong heat; at the ſame time the mixture diminiſhes conſiderably in its bulk.

Dr. Priestley ſuppoſes, that this diminution principally takes place in the common air. Not that the whole is to be aſcribed to it, but that the nitrous air likewiſe contributes in ſome degree. He proves this by the greater or ſmaller diminution, which he has experienced in the volume of theſe two kinds of air, according to the different proportions in which they were mixed. When, for inſtance, he mixed one meaſure of nitrous air with two of common air, after ſome minutes, when the effervеſcence was ended, the whole volume, inſtead of being three meaſures, which it ſhould have been, in proportion to the ſum of the volume, was found, on the contrary, a ninth leſs than the two meaſures, viz. leſs by a ninth in meaſure than the quantity of common air introduced into the mixture.

When,

When, on the contrary, he employed more of the nitrous than of common air, the volume of the mixture was lefs than that of the two feparately, but greater than that of the nitrous air alone. Thefe appearances Dr. Prieftley was able to explain on no other principles, than the fuppofing, that the greater diminution took place in the common air.

Dr. Priestley next proceeded to mix twenty parts of nitrous air with one part of common air. The diminution was about a fortieth, viz. half the bulk of the common air: for as we have feen above, that the diminution of common air, in all cafes, never exceeded from a fourth to a fifth, it follows that the remainder of the diminution muft be in the nitrous air.

The proportion of about two thirds of common air, to one third of nitrous will nearly form the point of faturation. After we have arrived at this point if we add more nitrous air, neither the rednefs nor effervefcence will be produced, and it makes an addition equal to its own bulk.

There is every appearance that the water which is ufed to confine the air in the receiver, when this mixture is formed, abforbs a part of the air; and in fact the diminution in the bulk is lefs when quickfilver is employed inftead of water. Two parts of common, with one part of nitrous air, give, in this cafe, by their combination, two parts and a feventh, inftead of a ninth lefs than two parts; if afterwards water be introduced into the receiver, it abforbs fome part of the air, but it never proceeds fo far as if the mixture had been, originally, made in water.

Nitrous air makes no effervefcence either with fixed or with inflammable air, nor in general with any air which has been reduced by any method whatfoever; neither do we remark any diminution in their bulk. On the contrary, the more falubrious the air, the more confiderable is the diminution of its bulk; and this circumftance has afforded Dr. Prieftley a certain method of diftinguifhing wholefome air from that which is not fo. From the moment he made this difcovery, he preferred this teft to that of experiments made on animals.

Nitrous air is susceptible of being absorbed by water, especially by such as is free from air. As to the quantity of this absorption, Dr. Priestley has given results which do not seem to agree exactly with each other*. When this air has been once combined with water it is difficult to separate them; a quantity of water, thus saturated, yielded a few bubbles in an exhausted receiver, and though it was suffered to stand there, a long time, it still retained its peculiar taste †. Dr. Priestley has, however, found by experiment, that this water, when it had stood all night near the fire, had become quite vapid, and a filmy kind of matter had been separated, which he supposes to be a calx of the metal from which the air had been obtained. Water, impregnated with nitrous air, may be easily preserved in bottles, even uncorked, in a cold place. Dr. Priestley has never observed the least alteration in it.

* These differences depended on the variety in the circumstances under which the experiments were made, and are not to be attributed to any inaccuracy in Dr. Priestley's manner of conducting or relating them. T. H.

† Dr. Priestley has since found, that nitrous air has never failed to escape from the water, which has been impregnated with it, by long exposure to the open air. T. H.

“ The diminution of common air, by a mixture of nitrous air, is not ſo extraordinary, as “ the diminution which * nitrous air itſelf is ſubject to, from a mixture of iron filings and brimſtone, made into a paſte with water. This “ mixture, as has been already obſerved, diminiſhes common air between one fifth and one “ fourth, but has no ſuch effect upon any kind “ of air that has been diminiſhed and rendered “ noxious by any other proceſs, but when it is “ put to a quantity of nitrous air, it diminiſhes “ it ſo much that no more than one fourth of the “ original quantity will be left.”

“ The effect of this proceſs is generally perceived in five or ſix hours, about which time the “ viſible effervеſcence of the mixture begins;

“ and

* Mr. Lavoisier having miſtaken Dr. Prieſtley's meaning, in imagining that the Doctor had declared that the diminution of common air effected by means of brimſtone and iron-filings might be carried ſtill further by an addition of nitrous air to it; I have thought proper to ſubſtitute Dr. Prieſtley's own account inſtead of the paragraph which Mr. Lavoiſier intended as an abſtract of it. I have marked the quotation, ſo far as it proceeds, with inverted commas. T. H.

" and in very ſhort time it advances ſo rapidly, " that in about an hour almoſt the whole effect " will have taken place. If it be ſuffered to " ſtand a day or two longer, the air will ſtill be " diminiſhed farther, but only a very little far- " ther, in proportion to the firſt diminution. " The glaſs jar, in which the air and this mix- " ture have been confined, has generally been ſo " much heated in this proceſs, that I have not " been able to touch it."

" NITROUS air thus diminiſhed has not ſo " ſtrong ſmell as nitrous air itſelf, but ſmells juſt " like common air in which the ſame mixture has " ſtood; and it is not capable of being diminiſh- " ed any farther, by a freſh mixture of iron and " brimſtone."

DR. PRIESTLEY has attempted to mix nitrous with inflammable air, and the mixture was inflammable. The flame which proceeded from it, had this peculiarity, that it was of a green colour; this circumſtance, Dr. Prieſtley ſuppoſes to depend on the nature of the air itſelf, and not on the metal from which it has been extracted.

A very ſingular, and almoſt incredible, phenomenon was that nitrous air, whether by itſelf, or combined with common air, always retained a ſpecific gravity, ſenſibly equal to that of atmoſpheric air. Dr. Prieſtley in a quantity of three pints, never obſerved more than the difference of half a grain, which was ſometimes more, and at other times leſs. How is it to be conceived, however, that two fluids ſhould penetrate each other in ſuch a manner, that there ſhall reſult a diminution of a third of their bulk, without the ſpecific gravity of the mixture being greater, than that of each of the fluids had been ſeparately?

Nitrous air is exceedingly fatal to vegetables. Whether the air were pure, or mixed with common air to the point of ſaturation, the plants which were confined in it, periſhed in a ſhort time.

Metals, calcined in this air, produced no ſenſible effect. Dr. Prieſtley has diſcovered that its antiſeptic powers are much greater than thoſe

of

of fixed air, and that it is capable of preſerving fleſh, for a long time, from corruption.

Dr. Priestley terminates this article by a table of the quantities of nitrous air, obtainable from different metals; it appears from hence, that iron afforded the moſt, next to it braſs, then copper and ſilver. The other metals furniſhed much leſs *.

SEC-

* When Dr. Prieſtley publiſhed an account of his Experiments in the Philoſophical Tranſactions, he had endeavoured to determine whether lime-water was precipitated in the diminution of common by nitrous air, in order to know if the fixed part of the former were depoſited; but on putting a bottle of lime-water into the jar in which the proceſs was made, no precipitation of the lime enſued, though it was eaſily effected, afterwards, by breathing into it. This experiment not having ſucceeded, Mr. Lavoiſier has taken no notice of it. But Dr. Prieſtley has ſince informed us that when the whole proceſs was made in lime-water, the precipitation was ſufficiently ſenſible. Experiments and Obſervations on Air, p. 114.

The ſecond part of Dr. Prieſtley's Experiments and Obſervations on different kinds of Air contain many curious

SECTION VII.

Of air infected with the fumes of burning charcoal.

THE honourable Mr. Cavendiſh had informed us, in a memoir communicated to the Royal Society,

rious diſcoveries, but as it would exceed my limits to enter into a particular detail, I ſhall therefore confine myſelf to a general account of them. From the green powder which remained after the evaporation of a ſolution of copper in nitrous acid, he obtained by means of a burning lens a quantity of nitrous air. When any volatile alkali was added to a mixture of nitrous and common air while effervefcing, the jar was filled with white clouds which, precipitating in form of ſnow, made a moſt beautiful appearance, and proved to be a nitrous ammoniac. At the ſame time a diminution of the common air took place. So that the Doctor imagines the ſalt to have been formed by the acid of the nitrous air, let looſe in the decompoſition of it, by common air, while the phlogiſton, which muſt be another conſtituent part of nitrous air, entering the common air, was the cauſe of its diminution. And to this laſt cauſe he attributes all the diminutions of common by nitrous air.

Having

Society, which is inſerted in the Philoſophical Tranſactions, that air paſſing through a red hot iron tube, filled with the duſt of charcoal, was diminiſhed about a tenth part in its bulk; he had alſo obſerved that ſome fixed air was obtained in that proceſs. Dr. Prieſtley repeated theſe experiments, and with ſimilar ſucceſs.

Dr.

Dr. Priestley having placed ſome iron nails in a bottle of nitrous air, when they had ſtood with the mouth of the bottle immerſed in quickſilver above a month, the nitrous air was found to be transformed into a very ſingular kind of air, in which a candle burned naturally and freely, and yet it was, in the higheſt degree, noxious to animals: whereas, in general, animals will live in air in which candles have burned out. When the nitrous air was longer continued in theſe circumſtances, the flame of a candle became twice, and ſometimes five or ſix times larger in it, yet without any exploſion. Liver of ſulphur produced this effect on nitrous air much more expeditiouſly, and diminiſhed it even to nineteen-twentieths of its original quantity. Dr. Prieſtley did not proceed to determine whether it were capable of farther diminution. In this ſtate, and, in general, when the diminution had reached beyond three fourths of the original quantity, a candle would not burn in it. Agitation in water deprived it of its noxious qualities. In one inſtance nitrous air, in which iron had ſtood, fired and exploded like weak inflammable air.

Dr. Priestley varied the experiment, by repeating it under a receiver, by the aſſiſtance of the focus of a burning lens, and he was able to produce a diminution of one-fifth in the volume of air; the remaining four-fifths were partly fixed, and partly inflammable air*. It is very worthy of remark, in this experiment, that, *ſometimes*, though the charcoal which was uſed, had been prepared by a fire ſo ſtrong as to be capable of vitrifying part of the crucible in which it was confined; no diminution was perceivable

The electric ſpark taken in nitrous air diminiſhes it to one-fourth of its former quantity.

From all theſe, and many other concurring circumſtances, Dr. Prieſtley concludes that all the difference between freſh nitrous air, and the various ſtates in which he has deſcribed it, depends upon ſome difference in the mode of the *combination* of its acid with phlogiſton, or on the proportion between theſe two ingredients in its compoſition.

From the fatality of nitrous air to inſects, Dr. Prieſtley recommends the trial of it, diluted either with common or fixed air, in the form of clyſters, as a remedy againſt worms in the inteſtines. T. H.

* The fixed air Dr. Prieſtley ſuppoſes to have been depoſited by the common air, and not to have proceeded from the charcoal; the other part is not ſaid to have been inflammable. Prieſtley's Experiments p. 130. T. H.

ceivable in the volume of air in which it was burned *. Dr. Prieftley attributes this effect to the inflammable air which is detached from the charcoal in this laft cafe, and which fupplies the place of the air which is abforbed. He obferves, in fupport of this explanation, that charcoal which had not been made with a confiderable degree of heat feldom failed to yield a permanent addition of inflammable air, and fuppofes, that in converting dry wood into charcoal, the greateft part is changed into inflammable air. If inftead of burning the charcoal over water, the operation was performed in quickfilver, there was then no diminution, but rather an augmentation in the volume of air, either on account of the fixed air, which was feparated, or of the inflammable air, but principally of the former. When lime-water was afterwards expofed to this air, it immediately precipitated, and the air was diminifhed one-fifth. But it was a fingular circumftance, that the charcoal which Dr. Prieftley

 made

* In general, however, Dr. Prieftley feems to think, that the greater the heat, and the longer its continuance, the purer will be the phlogifton which the charcoal contains. T. H

made ufe of in this experiment, and which weighed exactly two grains, was found not to be fenfibly diminifhed in weight at the end of the operation.

When air has been reduced by the burning of charcoal, it extinguifhes flame, is mortal to animals, makes no effervefcence with nitrous air, and is capable of no farther diminution, either by burning frefh charcoal in it, by a mixture of brimftone and iron filings, or by any other known caufe of the diminution of air.

SECTION VIII.

On the effect of the calcination of metals, and of the effluvia of paint made with white lead and oil, on air.

FROM the experiments on burning charcoal, which we have juft reviewed, Dr. Prieftley was led to fufpect, that the diminution of air was the confequence of its having more than its ufual quantity

quantity of phlogifton. The calcination of metals afforded him another method of producing the fame effect, or, according to his ideas, an emanation of phlogifton. Accordingly he fufpended pieces of lead and tin in given quantities of air, and threw the focus of a burning mirror upon them. The air, in this procefs, was diminifhed one-fourth; the remaining portion did not effervefce with nitrous air, was deftructive to animals, like air in which charcoal had burned, and it was incapable of farther diminution by the mixture of iron filings and brimftone; it was rendered innoxious, and, in a great meafure, recovered the other properties of common air, by wafhing in water. Whether Dr. Prieftley calcined tin or lead, in making this experiment, he found no fenfible difference in the properties of the air. He obferved in both cafes, that the water acquired a yellowifh tinge, and that a thin, and whitifh pellicle covered both the furface of the water, and the fides of the phial in which the calcination was made.

If inftead of immerfing the mouth of the jar, which contained the metals, into common water, it was placed in lime-water, no precipitation followed *, but its colour, ſmell and taſte were ſenſibly changed. If, inſtead of lime-water, quickſilver was uſed, the air was only diminiſhed one-fifth inſtead of one-fourth, and upon water being admitted to it, no more was abſorbed.

IT appears that Dr. Prieſtley attempted to calcine metals in inflammable, in fixed, and in nitrous air, without being able to accompliſh it: but he remarks that they may be calcined in air in which charcoal can no longer burn.

DR. PRIESTLEY accounts for all theſe phenomena, by the emanation of phlogiſton; that ſubſtance which is ſeparated from burning charcoal, and from metals during their calcination, unites, according to his doctrine, with the air, and diminiſhes its volume. Water being, afterwards, agitated with this air, deprives it of its phlogiſton, and the air becomes reſtored to its natural ſtate. He, alſo, preſumes, that vegetation corrects noxious air, by abſorbing the ſuperabundant phlogiſton.

THESE

* THE fixed air being more ſtrongly attracted by the metallic cala, than by the lime. T. H.

These considerations led Dr. Prieftley to a method of explaining the caufe of the mifchief which arifes from frefh paint made with white lead and oil, which, according to his account, is an imperfect calx of lead; and having painted feveral pieces of paper and placed them under a receiver, a fourth or fifth of the air was found to be abforbed in twenty-four hours. The remainder refembled air in which metals have been calcined; it did not effervefce with nitrous air, it was no farther diminifhed by a mixture of brimftone and filings of iron, and it was eafily reftored by fimple agitation in water.

SECTION IX.

Of Air Extracted by Means of Spirit of Salt, or Acid Air.

Dr. PRIESTLEY has experienced, after Mr. Cavendifh, that the diffolution of copper, by fpirit of falt, yielded an elaftic vapour. He caught this vapour in a veffel filled with quickfilver, and inverted into a bafon of the fame; but having afterwards introduced fome water to it *, almoſt the whole of it diſappeared, and nothing remained but a portion of inflammable air.

† This air turns lime water white; but Dr. Prieſtley does not think that the milky colour is owing to the precipitation of the lime, but to ſome particular circumſtance, which he has not had an opportunity of aſcertaining.

The ſolution of lead in the marine acid is attended with the ſame phenomena: about three-fourths of the generated air diſappears, on contact with water; the remaining one-fourth is inflammable. In the ſolution of iron in the marine acid, one-eighth only of the elaſtic vapour diſappears on the admiſſion of water; and in that of tin, a ſixth, and of zinc, a tenth only. The remainder of the air, detached from iron, burns with a green or very light blue flame. Dr. Prieſtley thinks, that this vapour is really abſorbed by

* Dr. Prieſtley mentions that three-fourths diſappeared.

† This remark is omitted in Dr. Prieſtley's treatiſe on air, ſince publiſhed. T. H.

by the water, and he is perſuaded, that there is a point of ſaturation, beyond which, the water is not capable of receiving an additional quantity.

It is evident, from Dr. Prieſtley's experiments, that the air, which is treated of in this article, is merely ſpirit of ſalt reduced into vapours; in fact, an elaſtic vapour, entirely ſimilar to this, is obtainable by means of ſpirit of ſalt alone, without the neceſſity of making any metallic ſolution. It is therefore eaſy to judge, from hence, that water impregnated with this vapour, is nothing but ſpirit of ſalt, and that it has all the ſame properties *.

Dr. Priestley is ſatisfied that this elaſtic vapour is much heavier than common air. Two grains and one-half of rain water are capable of abſorbing

* Since the publication of Dr. Prieſtley's Memoir in the Philoſophical Tranſactions, he has diſcovered, that acid air may be obtained, by having recourſe to the proceſs by which the ſpirit of ſalt is originally made.——Vide Dr. Prieſtley's Experiments and Obſervations on air, page 229. T. H.

forbing three ounce meafures of it; after which the water is increafed double in weight, and one-third in its bulk. This vapour, we are told by Dr. Prieftley, has a very ftrong affinity with phlogifton, it attracts it from all other fubftances, and forms with it inflammable air. This circumftance led Dr. Prieftley to believe, that inflammable air confifts of an acid reduced to vapours united with phlogifton. He was confirmed in his opinion, from having poured fpirit of wine, oil of olives, and oil of turpentine, to this air, and having expofed charcoal, phofphorus, and even fulphur to it, he obtained inflammable air from them; this laft experiment feems to fhew, that the marine acid, in this cafe, has the power of decompofing fulphur.

Dr. Priestley alfo fufpended a piece of falt-petre in this air: it was prefently furrounded with a white fume, in the fame manner as if the air had been mixed with nitrous air: this experiment proves likewife, that the vapour of fpirit of falt is, in fome circumftances, ftronger than the nitrous acid, as it is able to decompofe it, and difpoffefs it from its bafis.

Most kinds of liquors very readily abforb the vapour of fpirit of falt; linfeed oil abforbs it more flowly than others, and becomes black and glutinous *.

* The fecond part of Dr. Prieftley's work contains, alfo a great number of experiments in which he united this acid air with feveral different fubftances, and with various fuccefs. Ether and acid air, when combined and fuffered to efcape into the open air, formed a vifible fume like a white cloud, having the fmell of ether, but peculiarly offenfive. Liver of fulphur did not unite with acid air, fo as to form inflammable air. It imbibed half of the air to which it was expofed; one-fourth of the remainder was imbibed by water, and the remainder extinguifhed a candle. This experiment, the Doctor thinks, feems to prove, that acid air and phlogifton may form a permanent air, not inflammable. And he fufpects, that it may be air in fuch a ftate as common air loaded with phlogifton, and deprived of its fixed air; or rather the fame thing as inflammable air, which has loft its inflammability by long ftanding in water.

Quick-lime being put to acid air yielded a portion of air which was ftrongly inflammable; a proof that fome part of the phlogifton, which efcapes from the fuel, in contact with which the lime is burned, adheres to it. But Dr. Prieftley, by no means, imagines the caufticity of quick-lime to depend on this circumftance.

The fpecific gravity of acid air, does not appear to be far different from that of the atmofphere.

SECTION X.

Miscellaneous Observations.

Dr. PRIESTLEY places under this head, ſome experiments which he could not arrange under the preceding diviſions. He filled a bottle with ſmall beer, and placed it under a jar ſtanding in water; during ſome of the firſt days there was an increaſe of the quantity of air, and afterwards a gradual diminution, which proceeded to about a tenth of the original quantity of air. The beer, after this period, was ſour; the air, which remained, extinguiſhed candles, but having mixed it with four times the quantity of fixed air, a mouſe lived in it as in common air.

Our author eſtabliſhes it, as a principle, that every kind of factitious air is noxious to animals, except that which is extracted from ſaltpetre by detonation, in which a candle not only burned*, but the flame was even augmented, with a kind of

* Subsequent experiments made it probable, that though a candle burned in the manner related, an animal would not have lived in it. Vide Prieſtley's Experiments and obſervations, page 245.

of hiſſing, when the air had been newly ſeparated. Without doubt it, at that time, ſtill contained ſome particles of nitre which were not decompoſed. Dr. Prieſtley having kept this air, for a year, found, at the end of that time, that it was extremely noxious to animals; but having waſhed it in rain water, it recovered its wholeſomeneſs, and efferveſced with nitrous air in the ſame manner as common air does.

Dr. Priestley has alſo tried the effect of the vapour of camphor, and of volatile alkali on animals. A mouſe being placed in a bottle filled with this air, was not very much incommoded; it ſneezed and coughed, eſpecially after it was taken out, but there did not remain any ſenſible injury.

This work is terminated by ſome very remarkable experiments on common air, agitated a long time with water; he inverted jars, filled with common air, into boiling water; in a ſhort time, four-ſevenths of that air was abſorbed; the remaining part extinguiſhed flame, but it was not detrimental

detrimental to animals. The quantities abſorbed were not always, exactly, the ſame, but this circumſtance, doubtleſs, depended on the ſtate of the water employed.

The air, of which a part had been thus abſorbed, could not eaſily be reſtored, even by the vegetation of plants.

Dr. Priestley has obſerved, that a pint of water from his own pump contained about one-fourth of an ounce meaſure of air; this air extinguiſhed candles, but was not fatal to a mouſe. He alſo kept common air in receivers for a very long time, with a view of being ſatisfied whether its remaining in a ſtate of ſtagnation would make any alteration in it. After ſeveral months he tried it, and found it as ſalubrious, as it was the moment he firſt confined it, and it fermented as ſtrongly with nitrous air.

Dr. Priestley's paper is followed by ſome experiments of Mr. Hey *, the object of which

* An ingenious Surgeon-Apothecary at Leeds in Yorkſhire.

is

is to prove that water impregnated with fixed air, ſeparated from chalk by oil of vitriol, does not contain any other part of the ingredients which have contributed to furniſh the air. This water does not change the colour of ſyrup of violets, whereas a ſingle drop of the vitriolic acid diluted in a pint of water, turned it, very ſenſibly, of a purpliſh caſt.

THIS water rather curdled a ſolution of ſoap in water; but this effect Mr. Hey attributes to the combination of the fixed air, with the cauſtic alkali of the ſoap, which occaſions a ſeparation of part of the oil. It alſo renders turbid a ſolution of ſugar of lead.

As a ſequel to theſe experiments, is added a letter from Mr. Hey to Dr Prieſtley, on the effects of fixed air, applied by way of clyſter, in putrid diſeaſes *.

CHAP.

* IT is with particular pleaſure that I can congratulate the public on the acquiſition of ſo valuable an addition to the materia medica. Dr. Percival has favoured the public with ſome caſes in which fixed air thrown into the inteſtines,

CHAPTER XVI.

EXPERIMENTS ON LIME, BY MR. DUHAMEL*.

I HAVE already declared, that I deferred giving an account of Duhamel's experiments on lime, that I might not interrupt the thread of my

* THIS Chapter is extracted from the memoirs of the Royal Academy of Sciences, for the year 1747.

teftines, by way of clyfter, produced confiderable benefit, and that gentleman's practice, and the obfervation of other ingenious phyficians have fince furnifhed fome additional cafes in which the good effects of this medicine were indifputable. It has alfo been applied with great fuccefs, as a fumigation to foul ulcers, and I have ufed it with the moft happy event, in putrid fore throats, and ulcerated mouths and gums, attended with putrid fymptoms.

THE inftrument which Mr. Hey ufed for conveying the fixed air, was that employed for tranfmitting the fumes of tobacco into the inteftines, one end of a bladder was tied to my narration, relative to fixed air. I now haſten to do juſtice to that celebrated academician; it is

to the tube of this apparatus, detached from the box, and the other end round the neck of the bottle, which contained the chalk and vitriolic acid. It is better to faſten one end of the bladder to a perforated cork *, and the other to a flexible leather tube, to which another bladder and clyſter pipe may be affixed. By this means the effervefcence may be kept up as long as the operator thinks proper, by uncorking the bottle, and adding more acid or chalk. And the ſecond bladder ſerves to unite the clyſter pipe and leathern tube. By this means I have injected fixed air into the bowels with great facility and convenience.

Since the publication of this memoir in the Philoſophical Tranſactions, Dr. Prieſtley, as I have before obſerved, has republiſhed it, with additions to each article, and one entirely new ſection, containing obſervations on alkaline air, which he procured, with the moſt eaſe and convenience, from the materials uſed for the production of the cauſtic ſpirit of ſal ammoniac. Many obſervations are to be met with in this part of the Doctor's work, and another part contains many ingenious queries, ſpeculations and hints. I have ſelected from that volume, ſuch paſſages as were neceſſary to elucidate Mr. Lavoiſier's de-

* *Vide Dr. Prieſtley's directions for impregnating water with fixed air.*

is well known that there are few branches of ſcience which have not been enriched by him.

Mr. Duhamel has remarked, that white marble calcined in a very ſtrong fire, loſt about a third of its weight; but ſtill it was not calcined quite to the center, and there remained a nucleus in the middle, which partook as much of the nature of marble as of lime. The lime-ſtone of Courcelles, from whence is brought almoſt all the lime which we uſe in our buildings, was not nearly ſo difficult to calcine, and it appeared, in general, that the calcination was more readily and eaſily performed, in proportion to the ſoftneſs of the ſtone. The courcelles ſtones loſe, by calcination, about eight ounces, four drachms, from a pound weight, viz. ſomething more than half their weight. Expoſed, afterwards, to the air, they crack, fall into powder, and regain, by degrees, part of the weight which they had loſt; but there ſtill want five ounces

tail of the firſt memoir, but it would be doing injuſtice to the ingenious author's performance to attempt any farther abridgement of it. T. H.

and an half in the pound, to make up the weight which they poffeffed before calcination.

Mr. Duhamel has made fome inquiries into the quantity of water neceffary to flake lime. He took fixteen ounces of Courcelles lime; he flaked it with water, till it was of the confiftence of a thin pafte, and he left it to dry in the air, after which it weighed twenty-fix ounces, and had therefore acquired an augmentation of ten ounces. This lime, having been kept, a very confiderable time, in the heat of a ftove, was not perceptibly diminifhed in weight.

The quantity of water abforbed by lime prepared from marble, is much more confiderable than that abforbed by the Courcelles lime.

Mr. Duhamel tried to drive off, by fire, the fame water which he had introduced into the lime, but he found great difficulty in performing it; and although he made ufe of a melting furnace, in which the fire was excited by a ftrong blaft, the lime, uniformly, retained an increafe of weight amounting to four drachms and an

 half

half per pound. It was, without doubt, occasioned by the remainder of the water which could not be separated. This lime was then in the state of quick-lime, and presented the same phenomena.

Mr. Duhamel's memoir, moreover, contains very numerous and interesting experiments on quick-lime, and its combination with acids; but as they would be foreign to my subject, I shall not enter into a detail of them here. I shall content myself with relating, that lime, combined with the three mineral acids, did not afford compounds different from those obtained from chalk, and all the other pure calcareous earths. Mr. Duhamel has remarked, that a quick and penetrating vapour is discharged, in all these combinations, which precipitates a solution of silver, and this circumstance added to its odour, induced him to suspect that it was the marine acid.

Mr. Duhamel closes this memoir, with a particular observation, which was entirely new at the time of its publication: he dissolved salt of of tartar in diſtilled water, and, on evaporation, he obtained cryſtals from it; from whence it is evident, that the original diſcovery of the cryſtallization of alkalis, belongs to Mr. Duhamel.

CHAPTER XVII.

OBSERVATIONS CONCERNING FIXED AIR, AND THE EFFECTS OF CERTAIN MINERAL WATERS, BY MR. ROUELLE, LECTURER IN CHEMISTRY AT THE ROYAL BOTANICAL GARDEN IN PARIS *.

FIXED air becomes daily the object of chemical, and ſtill more, of phyſical inveſtigation. The celebrated Mr. Hales, was in ſome degree, the

* The work which I am now publiſhing on Elaſtic Vapours and the Fixation of Air in bodies, was nearly finiſhed, and I was preparing to read it at the Academy when theſe obſervations of Mr. Rouelle made their appearance. As they are ſhort, and as they are, in other reſpects, very intereſt-

the firſt who has pointed out the road, by the work which he has left us on that ſubject. Meſſrs. Black and Macbride have added a very intereſting ſeries of experiments affording much light. And laſtly; Dr. Prieſtley at London, and Mr. Jacquin at Vienna, have ſo ably ſupported Dr. Black's doctrine, that this ſubject is become one of the moſt intereſting in chemiſtry and phyſics, from the immediate relation which this recently known ſubſtance may, and, muſt have with an infinite number of the phenomena of nature.

I SHALL confine myſelf, at preſent, to the relation which fixed air appears to have with certain mineral waters, and ſome grand phenomena in nature, and I ſhall relate, as ſuccinctly as poſſible;

intereſting and not very capable of abridgment, I thought that the public would approve of my giving them at large. I have therefore only tranſcribed, verbatim, the article, from Mr. Roux's Journal de Medicine, for the month of May laſt *, where theſe obſervations are printed; and it is no longer I, but Mr. Rouelle who ſpeaks in this chapter.

* *Mr. Lavoiſier's Work was publiſhed* 1774.

poſſible, ſome experiments, which give us a knowledge of its uſe, and of its effects on the iron which is found in theſe waters, and afford a ſolution of ſome facts which cannot, in my opinion, be explained without it.

Distilled water, river water, in ſhort, the pureſt waters, as Dr. Prieſtley has marked are eaſily impregnated with fixed air; and then they have the ſame taſte, the ſame flavour, and afford the ſame appearances, as thoſe mineral waters which are properly called acidulous. This has been, already, firſt completely demonſtrated by Mr. Venel*. The experiments in proof of it are

* Mr. Rouelle, in this place, and Mr. Lavoiſier in his account of Mr. Venel's experiments, page 33, have attributed the firſt diſcovery of the natural impregnation of water to this ingenious profeſſor. I ſhould think myſelf inexcuſeable, therefore, if I did not take this opportunity of doing juſtice to the merits of our very worthy and learned countryman Dr. Brownrigg, and this cannot be done more effectually than by tranſcribing a paſſage from Sir John Pringle's elegant *Diſcourſe on the different kinds of air, delivered before the Royal Society, November* 30, 1773. I ſhall

are known, and I have only repeated them, that they might lead me more certainly, to thoſe which

ſhall only obſerve, that the memoir containing the account of Mr. Venel's experiments was not read before the Royal Academy of Sciences till the year 1750, and that he has ſuppoſed the air, contained in the waters which have been called acidulous, to be common atmoſpherical air.

The learned Preſident having firſt informed us that he believes Dr. Seip of Pyrmont had, originally, ſuggeſted the notion concerning the impregnation of the mineral waters by the mephitis, in a treatiſe publiſhed in the German language, and afterwards in a communication to the Royal Society in the year 1736, though that author ſeems not to have rightly underſtood the nature of that vapour, proceeds in the following manner. T. H.

" Thus the fuller diſcovery of this principle we owe to " Dr. Brownrigg of Whitehaven, who about thirty years " ago, began clearly to unfold this myſtery. But thoſe cu-" rious papers were not then inſerted in the Tranſactions, " as the too modeſt author had requeſted a delay, till he " ſhould be able to make them more worthy of that ho-" nour." In that communication he remarks " that a more " intimate acquaintance with thoſe nitrous airs in mines " called *damps*, might lead to the diſcovery of that princi-" ple of mineral waters, known by the name of their *ſpirit*; " that which I afterwards made, and of which I ſhall proceed to give an account.

1ſt. Some diſtilled water was impregnated with fixed air after Dr. Prieſtley's manner. I then immediately took a bottle of it, to which I added a ſmall quantity of iron ore, of the nature of the eagle ſtone, finely powdered. This ore is not attracted by the magnet, at leaſt not ſenſibly

"that the mephitic exhalations termed the *choak damp*, he "had found to be a fluid permanently elaſtic; and from "various experiments, he had reaſon to conclude, that it "entered the compoſition of the waters of Pyrmont, Spa, "and others; imparting to them that pungent taſte, from "which they are denominated *acidulæ*, and likewiſe that "volatile principle, on which their virtues chiefly depend.

"In order to aſcertain a fact of ſo much conſequence, "Dr. Brownrigg took the opportunity, when at Spa ſeveral years, to make ſome experiments for this purpoſe; "when he had the ſatisfaction to find thoſe waters pregnant with the *artificial* or *factitious air* of Mr. Boyle, the "ſame with that of the ſuffocating *grotta* near Naples, "and the ſame with the *choak damp* of our coal-mines; for "as much as this air inſtantly extinguiſhed flame, and the "life of thoſe animals he had incloſed in it."

ſenſibly ſo. The bottle was corked in the cloſeſt manner poſſible, and then ſet by, inverted for twenty four hours*. It had then diſſolved a ſufficient quantity of iron, to produce with an infuſion of galls, a ſtrong vinous violet tint, rather tending to black.

The liquor which is prepared to precipitate the Pruſſian blue, or phlogiſtigated alkali, ſtruck with it a greeniſh blue colour, and after ſome days a portion of precipitate was formed, which was true Pruſſian blue.

This water thus impregnated with air having been boiled, loſt all its properties. It became turbid, depoſited an okery matter, and no longer produced the violet, green, or blue tint with galls, or the phlogiſticated alkali.

Exposed to the open air for ſeveral days, it loſt, likewiſe, all its properties, and preciſely in the

* If ſome iron filings be placed in the baſon, which is uſed in Dr. Prieſtley proceſs for impregnating water with fixed air, the water diſſolves a ſufficient quantity of iron during the time that the other operation is performing. T. H.

the ſame manner as thoſe mineral waters which Mr. Monnet calls ferrugineous.

I am not the firſt perſon who has thought of diſſolving pure iron into water by means of fixed air. Dr. Prieſtley informs us, *that his friend Mr. Lane has put iron filings into this mixed water, and that he made a ſtrong and agreeable chalybeate or iron water, ſimilar to ſome natural waters which contain iron in ſolution, by means of fixed air only without any acid.*

But it is well known that iron is very rarely found in the boſom of the earth, joined with all its phlogiſton, and that nature ſeldom has iron filings in her poſſeſſion. I therefore thought that I ought to direct my experiments to a martial ſubſtance more commonly to be found; and, for this reaſon, I preferred the iron ores of the eagle-ſtone kind, which are very plentiful, and to be met with every where.

2dly, A pint of diſtilled water, four grains of ſea-ſalt, with an earthy baſis, twelve grains of Epſom ſalt, and as much iron ore as you pleaſe,

for

for the water takes up but a ſmall quantity, not being capable of diſſolving more.

This water being impregnated with air, yields, with galls, a ſtrong violet, or red wine, color, and with the liquor of Pruſſian blue, a deepiſh green color tending towards blue.

3dly, Water, impregnated with air, to a pint of which twelve grains of ſea-ſalt, and eighteen grains of fixed mineral alkali, had been added, took up leſs iron than the preceding. The violet color, by the galls, and the greeniſh blue, by the phlogiſticated alkali were more pale and languid. It is true, that both theſe colors ſhewed themſelves, a little, after ſome time.

This water loſt, by boiling, the property of turning green with the phlogiſticated fixed alkali; but an infuſion of galls ſtill ſhewed ſome traces in it of iron.

River water impregnated with fixed air, and to which a ſmall quantity of iron ore was added, ſtruck,

ſtruck, with galls, a very deep violet color, and a beautiful blue with the phlogiſticated alkali.

Some of this ore being mixed with the ſame *pure* river water unimpregnated with air, and the bottle being well ſtopped, and frequently ſhaken up, at the end of twenty four hours the water yielded no ſigns of the preſence of iron, by either of the teſts.

Mr. Monnet, in his treatiſe on mineral waters, propoſes, as an approved method of making a chalybeate water without air; to incloſe freſh iron filings in a bottle, well corked, and to ſhake it frequently for ſeveral days.

I shall have an opportunity of ſpeaking, on another occaſion, of this manner of making ferrugineous waters without fixed air. There are ſeveral natural waters, in fact, which contain iron, without that intermedium, as Mr. Monnet has demonſtrated.

5thly, Pure Arcueil water, unimpregnated with air, to which the ſame ore had been added,

gave

gave no marks of the preſence of iron, when tried by the teſts. I furniſhed it with air, and the iron was preſently diſſolved: galls afforded me a violet colour, which ſhewed itſelf gradually; and the phlogiſticated alkali ſtruck, immediately, a deepiſh green colour.

SOME ſpirit of ſalt was added to this water, with an intention of ſaturating, in part, the abſorbent earth which it keeps in a ſtate of ſolution. I afterwards impregnated it with fixed air, and I obtained, by the teſts, the common violet, and blue or green colors; but each of them was leſs deep than in the preceding waters. It ſeems as if the preſence of the ſalts and earth, with which ſome waters are loaded, impedes greatly the ſolution of the iron. I have however found, that my own well water ſuſpends a ſmall quantity of iron without any air. This water, being boiled, depoſites all the iron, ſo as not to be affected by the teſts.

6thly, PURE Sein water, furniſhed with air, by the common apparatus, from the vapour which is ſeparated during the precipitation of

hepar

hepar ſulphuris, by acids, and mixed with the ſame ore, ſcarcely changed colour with infuſion of galls, and not at all with the phlogiſticated alkali. However, it muſt be obſerved, that not only iron ore, but alſo calcined ſaffrons of iron, not attractable by the magnet, as the ſaffron of the reſiduum of corroſive ſublimate, and that which is called the Berlin Red, turn black pretty readily when they are mixed with that water impregnated with this vapour.

Water furniſhed with this vapour, receives the taſte and a ſtrong ſmell of the hepar; and retains them both for a conſiderable time, even in the open air; but it grows turbid, and becomes like whey which has not been clarified; this is to be attributed to a very minutely divided portion of ſulphur, which ſeparates from the water and precipitates.

This vapour, which ariſes, in the precipitation of hepar by all the acids, is very inflammable*. It is ſtill ſo after having paſſed through water,

This Note is Mr. Rouelle's.

* I thought that I had been the firſt who had ſeen this phenomenon, but on recollection, Mr. Meyer has mentioned

water, with which it forms ſcarcely any union; which induces me to believe that it contains very little true and pure fixed air, although it be very copiouſly diſcharged by the efferveſcence of the acids with the alkali of the hepar; but I ſee from the phenomena which it preſents, that it is here, as in the ſolutions of the metals by acids, in

ed it. It was chance which offered it to him as well as to me. My brother and I were employed in the year 1774 to examine ſome gold coin which was ſuppoſed to be ſo alloyed, that none of the uſual methods uſed for the aſſaying and purification of gold could ſeparate it. We had at that time four ounces in ſolution by the hepar. The precipitation was made at night; the candle was near, and I beheld myſelf ſurrounded on every ſide with a large flame, of which I ſoon knew the cauſe. Mr. Meyer ſeems to attribute the inflammability of this vapour to a portion of real ſulphur, which is divided in ſuch a manner, that it is volatilized, and carried away with the torrent of the vapour, in which, I preſume, he was miſtaken. The vapour itſelf is inflammable, and any portion of ſulphur it may contain, burns with it, and is only an acceſſory to its inflammation; for if we agitate this vapour, thus loaded with ſulphur, in water, the ſulphur is ſeparated, as I have remarked above; but the vapour, though deprived of this foreign ſulphur, does not ceaſe, on that account, to be inflammable.

in a very different ſtate from common fixed air. Thus water is but ſlightly, and with great difficulty, impregnated with that vapour. Dr. Prieſtley has made the ſame obſervation.

7thly, I TOOK a pint of pure river water, I then added, according to Mr. Venel's proceſs, two drachms of fixed mineral alkali, and ſix drachms of ſpirit of ſalt, which from ſome previous experiments, was the quantity neceſſary to ſaturate that quantity of alkali. The bottle was cloſely corked during the time of efferveſcence. Twenty-four hours after I opened it carefully, that I might introduce ſome iron ore, and I corked the bottle again immediately.

WHEN forty-eight hours had elapſed, the water, ſtill, ſhewed marks of air both to the eye and taſte; but only became moderately brown with infuſion of galls, and ſcarcely turned green, ſome time after, by the addition of phlogiſticated alkali.

8thly, I CAUGHT, in a bladder, the vapour which ariſes from a ſolution of iron in ſpirit of falt. This vapour which is inflammable, and remains fo a long time, incorporates very difficultly with water; but however fmall the quantity may be which the water receives, it, neverthelefs, contracts a very fenfible odour like hepar fulphuris, or rotten eggs.

Water, alfo, abforbs but a very fmall quantity of the vapour which arifes on the folution of iron in the vitriolic acid; but it does not contract the fame fulphurous fmell as that in the laft experiment.

The air which is feparated from bodies, is, then, in two very different ftates. In fome, it is pure fixed air, which combines with water in a quantity at leaft equal to it in bulk, and communicates to it many properties, and among others thofe of diffolving iron, and of precipitating lime-water, in the fame manner as fixed air itfelf, &c. Such is the air which is detached by the combination of acids with alkaline and calcareous fubftances, the vapour which arifes from fpirituous liquors in actual fermentation, and

and that of charcoal. In all thefe cafes this vapour, or fixed air, is not inflammable.

On the contrary, that which is feparated in the precipitation of liver of fulphur, by any of the three mineral acids, or by the acetous acid, that which is plentifully fupplied from the folutions of iron and zinc in the vitriolic and alfo in the marine acids, are very inflammable. This vapour paffes through water without incorporating with it, and without lofing its inflammability, which it retains for a long time. It communicates to water very fenfibly the tafte and fmell which attends the precipitation of liver of fulphur. But yet it differs from common fixed air, in not precipitating lime-water; and to fpeak *en paffant*, it may be compared to the air obtained by the diftillation of animals and vegetables, which Dr. Hales firft examined, and which he has recorded as being inflammable a long time after.

Not that there is not much air difcharged both in the precipitation of hepar fulphuris, and in metallic folutions; but it is evidently combined with a great quantity of phlogiston, and it is in proportion to this combination, that the air is more or less immiscible or insoluble in water, and becomes inflammable.

If we now cast a view upon what passes in the great works of nature; I believe that the same difference exists between that incoercible being, if I may be allowed the expression, which flies off from the cold mineral waters, which are falsely called acidulous, as those of Bussans, Seltzer, &c. and the sulphureous vapour which arises from the warm waters, as those of Aix-la-Chapelle, Bareges, Canterets, &c. In the first it appears, that this being is nothing but fixed air, the same as is obtained by Dr. Priestley's method. Instead of which, the sulphureous vapour of the Aix-la-Chapelle waters, &c. must have a great resemblance to that which is separated in the precipitation of the hepars.

It might be hoped that the chemists, who are more nearly situated to those waters, would verify this conjecture, and also inform us whether that vapour be inflammable like this of the

hepars.

hepars. It is certain, that the latter has precifely the fame colour as that which proceeds from the mineral waters. It has alfo the property, even when introduced into water, of turning filver black; as well as the metallic calces, and even crocus martis perfectly calcined, and not attractable by the magnet.

We may alfo obferve the fame relations and the fame differences in mephitic exhalations, of which we know there are two forts. Thofe of one fpecies, fuch as thofe of the Grotto del Cani, are not inflammable; they neither make filver nor the metallic calces black; they extinguifh candles, &c. in the fame manner as the vapours which are difengaged in fpirituous fermentation. Thofe from charcoal, and the fixed air arifing from the combination of acids and alkali, after Dr. Prieftley's manner, produce the fame phenomena as the Grotto del Cani, and may be compared to it on every account.

There is then feparated from the earth a fixed air fimilar to that produced in certain chemical experiments, and in the fermentation of ſpirituous liquors; ſince this, as Dr. Prieſtley remarks, has alſo the property of diſſolving in water. It is principally on account of this air, that the cold mineral ſprings keep a larger quantity of iron diſſolved, and that after the manner of our artificial aërial waters, they readily depoſite it, either by ſtanding expoſed to the open air, or elſe by being boiled.

The volume of fixed air which is introduced into water, is, according to Dr. Prieſtley, equal to that of the water which is impregnated by it. This air is not merely interpoſed among the particles of water, but is in a ſtate of actual combination; and the water may even be filtered, without being ſenſibly deprived of it. However, the water does not acquire, by it, any increaſe either in bulk or weight, proportionable to the great volume of air which it has abſorbed.

May we not ſuſpect, from all the effects of fixed air, that it is this which paſſes from the earth in vegetation, by means of that motion of univerſal fermentation, which the return of the ſun excites in nature, in the beginning of ſpring?

In fact, the air which is combined with vegetables, according to Dr. Hales's experiments, has loſt all its elaſtic properties, although it be in quantity very large and ponderous.

As to the other ſpecies of mephitic exhalations, we know that there are ſeparated in the galeries of mines, and in coal, and ſalt-pits, &c. two ſorts of vapours, one of which is frequently even viſible. It is immiſcible in water, and takes fire, and explodes frequently with a loud crack. The other, on the contrary, is not inflammable, but extinguiſhes lamps and candles, like the vapour of the Grotto del Cani, like that of ſpirituous fermentation, and that from charcoal. But they are equally fatal to animals which are expoſed to them.

We know that there are vapours ariſing from certain waters, both ſubterraneous, and even above ground, which take fire and burn very rapidly.

Dr. Priestley has concluded, from ſome ſalutary effects of which he has been informed, that fixed air is not noxious, and may be refpired *. For my part, I ftrongly fufpect that wherever it be collected in quantity, and without communication with the air of the atmofphere, it may become dangerous, and perhaps fatal, like the vapours of which we have been fpeaking; of this I fhall give an account, from a train of experiments which may decide the queftion *.

As to the vapour of hepar fulphuris, I dare affert that it is alfo pernicious like that of charcoal. It was at my own expence I obtained this knowledge,

* Mr. Rouelle certainly miftakes Dr. Prieftley. The latter only meant that it might be refpired in fmall quantities and with caution; not that it was totally innoxious in whatever quantity it might be refpired. Dr. Prieftley's expreffion is, that "he was fatisfied that fixed air is not noxious *per fe* any more *than heat*;" but no one can fuppofe that heat, in the extreme, is innoxious. Directions to impregnate Water, &c. page 18. T. H.

† It appears that Mr. Rouelle, at the time of writing this Memoir, had feen no other part of Dr. Prieftley's work on this fubject except his Directions for impregnating Water with Fixed Air, for he has inferted a note in this place, in which he fays, that he is juft informed that Dr. Prieftley had decided the queftion. T. H.

knowledge, having been once nearly ſuffocated by it.

The following are the ſymptoms which that vapour occaſioned. Having been deſirous to reſpire it ſtrongly, to diſcover the character of this odour, I held my noſtrils and open mouth over the veſſel, while I made a very large precipitation of hepar. I was immediately affected by it, and ſuddenly found myſelf abſolutely unable to inſpire, and eſpecially to expire. I perceived my breaſt to be in a ſtate of dilatation, joined with an inſupportable oppreſſion. In this ſituation, notwithſtanding all my efforts, I could neither introduce freſh air, nor expel that already in my lungs. I directly threw myſelf out of the laboratory of the King's garden, where I made this experiment; I got into the open air, and ſupported myſelf by the court-wall, for I felt myſelf univerſally faint; and it was not till after I had made ſeveral very ſtrong efforts to reſpire in the open air, that I recovered that function, together with that of motion. But I continued all the afternoon in a ſtate of uneaſineſs and oppreſſion,

preſſion, attended with a weight in my head which I cannot eaſily deſcribe *.

It is known that fixed air, ſeparated after Dr. Prieſtley's proceſs, alſo poſſeſſes properties in common with ordinary air. If it be introduced into a vacuum, the vacuum is deſtroyed, and the veſſels are detached. The ſame phenomenon is produced by inflammable air. It is therefore able to counterbalance the preſſure of the atmoſphere; which, in my opinion, proves, among other things, that this vapour is not phlogiſton only or acidum pingue, as has been advanced on mere ſpeculation, but, on the contrary, that it is ſome kind of air, which, however combined, ſtill retains the principal properties of common air, although it may differ from it in other reſpects †.

CHAP.

* Mr. Meyer has related a ſimilar accident which happened to his aſſiſtant, in his preſence, when making a precipitation of a large quantity of hepar.

† I am juſt informed that an Engliſh diſſertation by Dr. Prieſtley has lately appeared, which contains a very beautiful

CHAPTER XVIII.

EXTRACT OF A MEMOIR, BY MR. BUCQUET, DOCTOR-REGENT OF THE FACULTY OF PHYSIC IN PARIS, ENTITLED, PHYSICO-CHEMICAL EXPERIMENTS ON THE AIR, WHICH IS SEPARATED FROM BODIES AT THE TIME OF THEIR DECOMPOSITION, KNOWN BY THE NAME OF FIXED AIR, READ AT THE ROYAL ACADEMY OF SCIENCES, APRIL 24, 1773.

MR. BUCQUET, after having given, in a very concife abridgement, an account of the experiments of Van Helmont, Boyle, Black, Macbride and Jacquin, on the nature of the elaftic emanations which are difengaged from bodies,

tiful feries of experiments on fixed air, inflammable air, and mephitic air or that arifing from putrefaction. I regret that I did not know of them fooner; the manner in which the experiments are made which we have already had from him, affures us of the excellent ufe which may be made of every thing which comes from his hands.

bodies, and on fixed air, attempts to determine, 1ſt, Whether fixed air be the ſame as that of the atmoſphere; 2dly, Whether it be the ſame from whatever bodies it may have been extracted.

Mr. Bucquet, in a great part of his experiments, made uſe of Dr. Macbride's apparatus, of which we have given a deſcription in another place*. It may be recollected, that it conſiſts of two bottles, between which a communication is made by a bent glaſs tube. This apparatus, as made uſe of by Dr. Macbride, has the great inconvenience of not permitting us to operate on fixed air without a very conſiderable quantity of atmoſpheric air being mixed with it, and this circumſtance induced Mr. Bucquet to make ſome alterations in it. He added cocks to it, he diſpoſed it in ſuch a manner that he could adapt it to an air pump; laſtly, he cut one of the bottles through the middle, ſo that the upper part might be ſeparated, and an accurate barometer be introduced. That which was deſigned to receive the ſubſtances which were to be combined together

* Chapter IX. page 52.

ther to produce the air, Mr. Bucquet called *the bottle of mixture*, and that which was to receive the bodies which he propoſed to expoſe to the vapour of the ſeparated air, he named *the bottle of reception.*

The reſult of the experiments, made with this apparatus, was that the air ſeparated by all the acids, without exception, whether from chalk or alkaline ſalts, was abſolutely the ſame; he only obſerved that the air drawn from volatile alkalis, retained a ſmell like that of ſtinking fleſh. He alſo found a perfect ſameneſs between the air detached from fermenting, and that from effervеſcing ſubſtances. This air has a penetrating ſmell, which Mr. Bucquet calls *odeur gaſeuſe:* it has the property of precipitating lime diſſolved in water, of changing it into calcareous earth, and of reſtoring to it the property of effervеſcing with acids. It produces nearly the ſame effects on the cauſtic alkalis; it reſtores to them the power of effervеſcing, and of cryſtalliſing.

Fixed air, in all theſe caſes, does not contain any

any thing of the ſaline ſubſtances from which it has been obtained. Syrup of violets, though expoſed to its action, for above twelve hours, in the apparatus which has been juſt deſcribed, was in no wiſe altered.

Mr. Bucquet ſubmitted this air to the uſual experiments, to determine its weight and compreſſibility; his reſults did not differ ſenſibly from thoſe which are obtained when common air is employed.

Mr. Bucquet afterwards examines the air produced by the ſolution of metallic ſubſtances, and he found it very different from that which is ſeparated either by effervefcence (from calcareous earths and alkalis) or by fermentation. This air is not at all ſuſceptible of being combined with water; it is equally incapable of combination, either with lime or with the cauſtic alkalis; however long a time they were expoſed to its action, they did not recover the property of efferveſcing with acids.

Fixed air diſcharged from an efferveſcing mixture,

ture, combined afterwards with wine, does not convert it into vinegar; it only communicates to it an acerb tafte, which may however be the firft ftage of the acetous fermentation.

Mr. Bucquet then proceeds to examine, if the air produced, either by effervefcence or fermentation be inflammable, like that feparated from zinc or iron, by the vitriolic or marine acids, as Dr. Hales has advanced; but he was not able to make it burn.

From thefe experiments, Mr. Bucquet concludes, that air produced, either by effervefcence, fermentation, or metallic folution, is not precifely the fame as that of the atmofphere, though equal in weight and elafticity; that air produced by effervefcence and fermentation differs from that of the atmofphere, and that of metallic folutions, in the very great aptitude which it has to combine with lime, with alkalis, and even with water; and laftly, that the air from metallic folutions has the diftinguifhing character of being inflammable.

Though

Though theſe experiments be very ſimilar to thoſe which had been publiſhed before Mr. Bucquet's, and eſpecially to Dr. Prieſtley's, they are not leſs valuable in phyſics. Experiments on a ſubject ſo intricate, and which is ſtill left in obſcurity, cannot be too much multiplied. It is alſo of ſome importance to know that we may arrive at the ſame reſults by different proceſſes.

CHAPTER XIX.

An APPENDIX on FIXED AIR, by Mr. BAUME, MASTER-APOTHECARY at PARIS, of the ROYAL ACADEMY of SCIENCES *.

SOME philoſophers are of opinion, that there are properties diſcovered in fixed air, that ſhould

* The apprehenſion that I might be accuſed of having ſhewn a ſpirit of partiality, in having brought to view, as I have,

ſhould lead to the rejection of phlogiſton, and the ſubſtitution of fixed air, in its place *. According to theſe philoſophers, fixed air ought to bring about a total revolution in chemiſtry, and change the order of all the knowledge which has been acquired. But the experiments which have been publiſhed, hitherto, have appeared to me to afford phenomena, concerning the cauſe of which, I apprehend, miſtaken notions have been received, as it will be eaſily judged by the following reflections.

We have proved in many places of this work, and agreeably to the moſt celebrated philoſophers, that air is an element which enters into the compoſition of moſt bodies. Hales, in his Vegetable and Animal Statics, has demonſtrated this truth by a great number of well executed experi-

* We are ignorant who theſe philoſophers are.

I have, whatever has been hitherto written on the ſubject of fixed air, has engaged me to tranſcribe, here, this appendix, which is to be found at the end of the third volume of Mr. Baumé's Chemiſtry, page 693.

experiments. He has calculated the weight and the volume of air contained in different bodies, and he has given the name of fixed air to that which enters into their compoſition; that, in ſhort, which is become one of their conſtituent principles, and which has loſt its elaſticity and all the properties of pure and aggregated air; and he has given the name of elaſtic air to that which is diſengaged from bodies.

AIR, as we have obſerved, under that head, is one and the ſame: there is only one ſpecies of air. This element may, and in fact does enter into an infinite number of combinations; but when it is diſengaged from bodies with which it was combined, it recovers all its properties, and when it is properly purefied it in no wiſe differs from that which we reſpire.

WHAT many chemiſts call fixed air at preſent, appears to be that which has been ſeparated from bodies by different methods: but it ought rather to be denominated, as Dr. Hales has called it, detached or elaſtic air. In fact, air thus ſeparated from bodies is no more fixed than that

which

which we breathe, ſince it recovers all its elaſtic properties as that philoſopher has demonſtrated.

AIR, as we have declared in many parts of this work, not only diſſolves water and is ſaturated with it, but it alſo diſſolves oily ſubſtances, &c.

WHEN air has been *detached* from bodies, by ſubmitting them to diſtillation in an apparatus ſimilar to that deſcribed by Dr. Hales, real philoſophers call it *fixed air*. This air, in diſengaging itſelf from the bodies, to which it was united, carries along with it different ſubſtances which it actually holds in ſolution, and properties are attributed to it, which do not belong to the air, but merely to the foreign ſubſtances with which it is loaded. It appears that this diſtinction has not been made, which however ought to preſent itſelf naturally.

WHEN an acid is combined with a calcareous earth, with an alkaline ſalt or with a metallic ſubſtance, there is ſeparated, as we have remarked, a conſiderable quantity of air and of fire in

 a ſtate

a ſtate nearly pure, which could by no means enter into the compoſition of the neutral ſalt which reſults from this union. If we collect, by a ſuitable apparatus, the air which is detached while this combination is formed, the air thus detached is ſtill named *fixed air*. Properties are found in this air different from atmoſpheric air, and it is therefore concluded that fixed air is not the ſame in all bodies; but the different properties which are diſcovered in it, ought to be attributed, as we have juſt ſaid, to the foreign ſubſtances with which it is loaded.

Air which is ſeparated from bodies, during either the ſpirituous, acetoſe or putrefactive fermentation, is alſo denominated *fixed air*; and theſe ſeveral kinds of air differ from each other, like the bodies from which they have been produced. Theſe obſervations alone might be ſufficient to ſhew that theſe different properties ought to be attributed to the matter with which the air is impregnated, and not to the air itſelf, which is an element incapable of any alteration. But inſtead of making theſe reflections, there appears a diſpoſition to eſtabliſh as many ſpecies

of

of air, as there are bodies which can ſupply it*, which can anſwer no purpoſe, but to obſcure the theory of chemiſtry. Some perſons are already willing to admit of fixed air which is inflammable; of fixed air which reduces the metals to a calx, and cauſes the augmentation in their weight; of antiſeptic fixed air, which reſtores putrefied fleſh to ſweetneſs, &c. &c.

THERE is not the leaſt doubt, but when an highly rectified oily ſubſtance is diſſolved by air, and is collected in a proper ſpace, it will burn, as Dr. Hales has remarked in many parts of his Vegetable Statics, and particularly in his analyſis of peaſe, oyſter ſhells, amber and wax, though he afterwards waſhed the air, ſeparated from theſe ſubſtances, eleven times. Oily ſubſtances, thus diſſolved by the air, or reduced to the ſtate of vapours, almoſt always take fire, with an exploſion, on the approach of a candle; but it is, by no means, the air which burns that element is not combuſtible.

 IT

* MR. BAUME certainly makes the advocates for fixed air, appear as holding abſurd notions, which never entered into their heads. T. H.

It has been a received doctrine among chemists, that metals are reduced to calces, only by the loſs of a portion of phlogiſton, and that they recover their metallic brilliancy, ſuch as before calcination, whenever this inflammable principle is reſtored to them; but ſome philoſophers, partiſans of fixed air, aſſert, on the contrary, that it is to the air which becomes fixed in the metal, during its reduction into a calx, that this change of ſtate and the cauſe of the augmentation in weight ought to be attributed. Theſe philoſophers ſuppoſe, that by detaching from the metallic calces the fixed air with which they are loaded, they may be reſtored to their metallic form, without any addition, even without fire; but this opinion relative to their reduction appears founded on miſtake, as phlogiſtic vapours are, imperceptibly, made uſe of in theſe operations.

We have ſaid, under the article of liver of ſulphur precipitated by an acid, that the vapours which ariſe from it are not at all inflammable, but that they reſtore without fire the metallic calces to their brilliancy. It is, by no means, the

the air which produces this effect, but merely the phlogiftic principle which it contains.

With regard to fixed air as an antifeptic, it is very probable, that there are many fubftances poffeffing antifeptic properties, which air may diffolve, and which may alfo counteract putrefaction, fuch as the Peruvian bark and other aftringent bodies, which alfo have antifeptic qualities when immediately applied to putrefied flefh.

From thefe reflections we may conclude, 1ft, That what is called *fixed* air is improperly named; the appellation of *detached* or *elaftic* air, which Dr. Hales has given it, is better adapted to it.

2dly, That fixed air, under this denomination which has been given to it, is common air, but impregnated with foreign fubftances which it holds in folution; air, which may frequently be purefied and reftored to the ftate of good air, fimilar to that of the atmofphere, by caufing it to pafs through different liquors, proper for filtering it, and for retaining the foreign matters, which deftroy its purity.

3dly, FIXED AIR, according to this theory, ought no longer to be examined, in the point of view in which it has hitherto been conſidered, but merely with reſpect to the ſubſtances which air is capable of diſſolving or of being impregnated with.

4thly, THERE is therefore a field for a curious train of experiments which may acquaint us what ſubſtances may be diſſolved in air, and what may be the properties of theſe ſubſtances when reduced to this ſtate. Theſe experiments made, in this point of view, would lead to more clear and certain knowledge, than any which have hitherto been publiſhed.

5thly, THE caſe is the ſame with air as with water; both elements have the quality of diſſolving, and being ſaturated with ſeveral ſubſtances; each of theſe elements acquires new properties which belong neither to water nor to air, but merely to the ſubſtances with which they are impregnated. As there are certain bodies which water is capable of diſſolving, and which cannot be ſeparated from it; the ſame ſhould be the caſe with air: this laſt element

may

may be impregnated with ſubſtances as volatile, and as dilatable as itſelf, and which can never be ſeparated from it, either by diſtillation, filtration, or any other method; but it does not the leſs follow, but that theſe new properties which are to be found in the air, ſhould always be attributed to the foreign ſubſtances, and not to the air itſelf *.

* These nineteen Chapters contain all that is moſt intereſting relative to fixed air, which I have been able to obtain. I might have added here an extract from a very well written Theſis, in favour of Dr. Black's theory, which was maintained at Edinburgh, September 12th, 1772, by Mr. Rutherford; but as it contains only a ſummary of what has been written on this ſubject by Meſſrs. Black, Cavendiſh and Lane, I was apprehenſive it might lead me into unneceſſary repetitions.

I am alſo acquainted that there has appeared lately a Collection of Chemical Diſſertations, by Dr. Wiegel, a Phyſician at Grieſwald, in one of which he treats of fixed air and of the acidum pingue; but it has not been in my power to procure that work.

END of the FIRST PART.

THE

SECOND PART.

NEW INQUIRIES RELATIVE TO THE EXISTENCE OF AN ELASTIC FIXABLE FLUID IN CERTAIN SUBSTANCES, AND THE PHENOMENA RESULTING FROM ITS DISENGAGEMENT, OR ITS FIXATION.

CHAPTER I.

Of the EXISTENCE of an ELASTIC FIXABLE FLUID in CALCAREOUS EARTHS, and the PHENOMENA resulting from the absence of it in LIME.

HAVING related, in the firſt part of this work, the opinions of Meſſrs. Black, Meyer, and de Smeth, on the cauſes of the cauſticity of quick-lime, and alkaline ſalts. It appeared neceſſary to me, before I proceeded farther, to traverſe again the ſame ground, to repeat the principal experiments of Meſſrs. Black, Meyer, Jacquin, Crans, and de Smeth, afterwards to add ſome new ones of my own; and, laſtly, to endeavour to fix, if poſſible, the ideas of of philoſophers as to the merit of theſe different ſyſtems.

Such is the object which I propoſe to complete in the three firſt chapters of this ſecond part. As the experiments which are here related, are all cloſely connected with each other, the unremitted attention of the reader will be requiſite.

EXPERIMENT I.

The Solution of Chalk in the Nitrous Acid.

I POURED into a ſmall matraſs, with a long narrow neck, ſix ounces of the nitrous acid, the weight of which was to that of water as 129,895 to 100,000. I added, by degrees, ſome chalk in powder dried in a degree of fire, long continued, nearly equal to that in which mercury boils.

The

The solution was effected by a quick effervescence, but with scarcely any heat. I took care to keep the matrass as much stopped as possible; only opened it from time to time, to give vent to the vapours which were discharged impetuously; these precautions were taken that the evaporation might be as small as possible. Two ounces, three drachms, and thirty-six grains of chalk were employed to saturate the acid; the whole weight, therefore, of both substances, amounted to eight ounces, three drachms, and thirty-six grains: but having weighed them again, after their combination, the weight was no more than seven ounces, three drachms, and thirty-six grains; which made the loss in weight exactly one ounce. This could only be attributed to the elastic fluid which was separated, and the watery or other vapours which it had carried off with it: a method was, therefore, to be discovered to retain and examine them. This I proposed to do by the following experiment.

EXPE-

EXPERIMENT II.

To Measure the Quantity of the Elastic Fluid, separated during the Solution of Chalk in the Nitrous Acid.

A. B. Figure 1ſt, is a braſs diſh of ten inches diameter; in its center, at C. is a ſhaft which ſupports another diſh, E. F. round like the former, and of 5½ inches diameter. On this ſecond diſh another ſhaft is erected, G. H. which ſupports a frame repreſented ſeparately in figure ſecond. This frame is deſigned to bear a glaſs phial I. in form of a pear. It ſhould have a ſpout to prevent the fluid, which it is deſigned to pour, from trickling down its outſide; in default of a ſpout, one may be made of wax. The phial I. inſtead of being ſuſpended by two pivots, ought rather to be ſuſtained by two hemiſpherical leather caps, with a ſcrew, which may ſeparate or bring them nearer according as the phial employed is more or leſs thick. The inferior part K. of this phial, ought to be ballaſted with lead, in order to keep it ſtraight. It ſhould alſo have

have in this part a button K. to which a thread ſhould be connected. This laſt ſhould paſs above the frame, and be introduced through the hole M. in the lower diſh, which is furniſhed with a ſmall pully. This thread ſerves to make a ſwing for the bottle I. when it is thought proper. All this apparatus is covered with a large receiver N. N. O. O. of ſix inches diameter, and from two and a half to three feet high. Laſtly, the whole ſhould be placed in an earthen-ware ciſtern V. V. S. S. of one foot diameter in its bottom, and nearly of the ſame height. It is repreſented as tranſparent in the figure, that the whole detail of the apparatus may be better underſtood.

When this machine is to be made uſe of, a certain quantity of acid, or any other liquor is to be placed in the bottle I. In the baſon Q. is placed ſome chalk, alkali, or other ſubſtance of which you would make your ſolution. The ciſtern V. V. S. S. is to be filled with water. The water is then to be raiſed by ſuction, by means of the hole R. at the top of the receiver, and to be made to mount nearly to Y. Y. higher or lower according to the nature of the experiments which are propoſed to be made. In fine, by means of the funnel repreſented in figure third, ſome oil is to be introduced into the receiver. The oil being lighter than the water mounts to its ſurface Y. Y. and by its interpoſition prevents the elaſtic fluid, ſeparated in the combination of the acid and alkali, from being abſorbed by the water. As ſoon as every thing is thus prepared, the thread R. is to be drawn, which paſſes through the three pullies marked *p*. M. *n*. and gives a ſwing to the bottle I.

To this apparatus may be joined a pump, and this precaution is even indiſpenſably neceſſary where ſubſtances are employed, whoſe vapours are noxious, and will not permit the air to be ſucked without inconvenience. Such a pump is repreſented, adapted to the apparatus in figure firſt. P. P. repreſents the body of the pump, Z. the ring which ſerves to raiſe the piſton. At each ſtroke the air is drawn in by the pipe X. L. whoſe extremity X. muſt riſe within ſome lines of the diſh: it is then filled again, and the air driven out through the body of the

pump

pump by the pipe T. As the lower extremity L. of the pipe X. L. as well as the extremity *s.* of the pipe which ſuſtains the pump, is deſigned to ſtand in the water, the ſcrews in thoſe parts ſhould be guarded with leather well greaſed. We ſhall ſee in the ſequel, other uſes to which this pump may be applied.

Into the bottle I. fig. 1ſt. I put an ounce and half of the ſame nitrous acid as that employed in experiment the firſt. In the baſon Q. were placed four drachms and ſixty three grains of the ſame chalk, and dried in a degree of heat in which mercury boils. The water was raiſed up to Y. Y. as above mentioned, and I introduced a covering of oil upon the ſurface of the water, I then mixed the acid and chalk by means of the ſwing, taking care to do it gradually, leaſt by the briſkneſs of the efferveſcence the liquor ſhould run over the ſides of the baſon.

The water ſank ſuddenly in the receiver N. N. O. O. and ſettled ſeven and a half inches beneath the ſurface Y. Y. The receiver in this place was ſeventy lines $\frac{85}{100}$; whence it follows

 that

that the quantity of elaſtic fluid ſeparated, amounted to two hundred and ſix cubic inches; but in about a quarter of an hour, the ſmall degree of heat raiſed in the combination being diſſipated, the quantity of elaſtic fluid was reduced to two hundred inches. After which there was no farther ſenſible variation, even for ſeveral days. The thermometer * during this interval of time, ſtood from ſixteen to ſeventeen degrees, and the barometer at about twenty-eight inches.

The quantities of nitrous acid and chalk employed in this experiment were only a fourth of thoſe uſed in the former one; whence it follows that if ſix ounces of the nitrous acid, and two ounces three drachms and thirty-ſix grains of chalk had been employed, as in the firſt experiment, there would have been a ſeparation of eight hundred cubic inches of elaſtic fluid: but the loſs of weight in the firſt experiment was exactly one ounce; therefore eight hundred cubic inches of elaſtic fluid, ſuch as is ſeparated from

* Reaumur's thermometer was uſed in all theſe experiments.

from chalk, loaded no doubt with a large quantity of watery vapours which are carried off with it, weigh exactly one ounce, in a temperature from fixteen to feventeen degrees of the thermometer. A cubic foot, therefore, or one thoufand feven hundred and twenty-eight cubic inches of this elaftic fluid, weighs two ounces, one drachm and twenty grains; but a cubic foot of *common* air in that temperature does not weigh, according to the obfervations of Mr. de Luc, more than one ounce, two drachms and fixty-fix grains*. From whence we may already draw one or other of thefe conclufions, either that the elaftic fluid difcharged from chalk by effervefcence weighs about one-third more than common air, or, what is more probable, that by the violence of the effervefcence, it carries off with it a confiderable quantity of watery or other vapours, which contribute to increafe the lofs of weight obferved in experiment firft, and which occafion the elaftic fluid to appear heavier than it really is.

* The French drachm is divided into 72 grains.

EXPERIMENT III.

To determine the Proportion of Water necessary to saturate a given Quantity of Quick-Lime.

I put into an iron kettle twenty-eight ounces and ſix drachms of quick-lime, and by degrees poured on it a ſufficient quantity of water to reduce it to a paſte of a moderate conſiſtence. After the ebullition was over, the kettle was placed on a gentle fire, to evaporate the ſuperabundant moiſture. Care was taken to ſtir the mixture frequently with an iron ſpatula, to prevent its uniting and forming into large maſſes; towards the end the fire was increaſed, and gradually raiſed to a degree equal to that of boiling mercury; and it was kept up to this for ſeveral hours. At length, when the matter appeared to be perfectly dry, I withdrew it from the fire, and weighing it, while hot, the whole weight was found to be exactly thirty-ſeven ounces. I then quickly reduced into powder all the lime, in in a mortar which was kept continually hot; it was paſſed through a ſilken ſieve, and kept cloſely ſtopped in a glaſs bottle for uſe.

It appears from this experiment, that the relative weight of quick lime to that of ſlaked lime is as 1000 to 1287; that is to ſay, that 1000 parts of quick lime can abſorb $\frac{287}{1000}$ of water, or, in other words, that every pound of this ſubſtance can imbibe four ounces, four drachms and fifty-three grains of water.

It might perhaps be imagined that the lime not only abſorbs the water during its extinction, but that the air itſelf, or ſome matter ſuſpended in the air, combines with it during the operation, and contributes to the augmentation of weight which has been obſerved. The following experiment will deſtroy theſe conjectures, and make it evident that the external air has no concern in the phenomena attending the extinction.

EXPERIMENT IV.

Lime slaked in the Vacuum of an Air Pump.

An ounce and half of quick lime broken into pieces of a moderate ſize, was placed in a glaſs veſſel, and a ſufficient quantity of water poured on it; after which the veſſel was placed under the receiver of an air pump, and a vacuum made as quickly as poſſible.

The phenomena attending the extinction were in no wiſe different from thoſe which were obſerved in the open air. During a ſpace of ſome minutes the mixture ſwelled, attended with ebullition and heat. The lime was reduced to a white paſte, which was dried, and found to have received an increaſe of weight nearly equal to that obſerved in the former experiment. I do not deny, that the lime might abſorb a little elaſtic air during the operations of ſlaking and drying it; but the quantity is very inconſiderable,

able, and almoſt nothing in compariſon of the quantity of water which it abſorbs.

EXPERIMENT V.

THE SOLUTION OF LIME IN THE NITROUS ACID.

INTO a ſmall matraſs, with a long narrow neck, were poured ſix ounces of the ſame nitrous acid as that uſed in the former experiments. I then added, by degrees, ſome of the ſlaked lime ſaturated with water and dried as in experiment the third.

THE firſt portions were diſſolved with ſcarcely any motion, but as the acid became more ſaturated, the efferveſcence grew porportionably greater; but yet this efferveſcence was different from that ariſing from the ſolution of chalk: the bubbles were frequent, but ſmall, and the ſwelling inconſiderable; the heat, on the contrary, was very ſtrong, and even ſuch, that it appear-

ed

ed as if the phenomena of boiling were joined to thofe of effervefcence. The quantity of lime neceffary for faturation was one ounce, five drachms, and thirty-fix grains; the weight of the fame materials, after combination, was found to be feven ounces, four drachms, and feventy grains. The lofs of weight, therefore, was only thirty-eight grains.

It was of fome importance to compare, as in experiment the firft and fecond, the lofs of weight obfervable during the effervefcence with the quantity of elaftic fluid which is feparated. To effect this, the apparatus ufed in experiment the firft was employed in manner following:

EXPERIMENT VI.

To determine the Quantity of Elastic Fluid which separates from Lime during its Solution in the Nitrous Acid.

IN the bottle I. fig. 1ft. was placed an ounce and a half of the nitrous acid, before employed, and

and three drachms, twenty-ſeven grains of lime, ſlaked and dried (which appears to be the proportion neceſſary to ſaturate that quantity of acid) were put into the baſon Q. The reſt was diſpoſed as in experiment the ſecond.

The combination was formed, as in the preceding experiment, with a little motion of effervéſcence or ebullition. In the firſt inſtant, the water deſcended ſuddenly three or four inches in the receiver N. N. O. O. but in a few ſeconds it recovered its level, and ſettled about an inch below the former mark. The receiver was lukewarm, and as it contained a conſiderable body of air, a very ſenſible dilatation would naturally reſult, accordingly in proportion as that air returned to the ſame degree of heat with that of the laboratory, the water remounted, and the ſeparated air was found reduced to a cylindrical ſpace of four lines in height, by ſeventy lines $\frac{42}{100}$ in diameter, i. e. of nine cubic inches. Had this experiment been made with the ſame quantities of materials as in experiment the fifth, there is no doubt but a quantity of elaſtic fluid, four times as great, would have been ſeparated,

i. e.

i. e. thirty-ſix cubic inches. But ſuppoſing, as we may here do without any ſenſible error, that this fluid was exactly equiponderant with air, theſe thirty-ſix inches ſhould, in the temperature of the laboratory, weigh $16\frac{1}{3}$ grains. The total loſs of weight in experiment the fifth was but thirty-eight grains; from whence it follows, that notwithſtanding the great heat undergone during the diſſolution, the loſs of weight cauſed by the evaporation was only twenty-one grains two thirds.

General concluſions deducible from the ſix preceding Experiments.

IT is immediately evident from the third experiment, 1ſt, That the quantity of one ounce, five drachms, and thirty-ſix grains of ſlaked lime, employed in experiment the fifth, and neceſſary to ſaturate ſix ounces of nitrous acid, contained three drachms and three quarters of a grain of water. 2dly, From experiment the ſixth it appears, that the ſame quantity of ſlaked lime contained ſixteen grains and a half of elaſtic fluid. It contained therefore only, in reality, one ounce,

ounce, two drachms, eighteen grains, and three quarters of alkaline earth; but in experiment the firſt, two ounces, three drachms, and thirty-ſix grains of chalk, were neceſſary to ſaturate an equal quantity, viz. ſix ounces of the nitrous acid; from whence it ſeems, that we may conclude, that two ounces, three drachms, and thirty-ſix grains of chalk, in like manner, do not contain more than one ounce, two drachms, eighteen grains and three quarters of alkaline earth; and that they contain beſides, three drachms and three quarters of a grain of water, and ſix drachms, ſixteen grains and a half of elaſtic fluid, which ſix drachms, ſixteen grains and a half, by experiment the fifth, are equivalent to eight hundred cubic inches. From whence it follows, that a cubic inch of the elaſtic fluid contained in chalk weighs $\frac{561}{1000}$ or ſomething above half a grain, in a heat of ſixteen or ſeventeen degrees of Reaumur's thermometer. Whereas a cubic inch of common air, in the ſame temperament, weighs, according to Mr. De Luc, no more than $\frac{455}{1000}$ or ſomething leſs than half a grain. This difference happens either becauſe the elaſtic fluid diſcharged from chalk

chalk is actually a little heavier than atmospheric air; or becaufe it is charged with vapours in its feparation from the chalk; or, laftly, becaufe chalk contains more water than flaked lime.

If, as we have juft faid, two ounces, three drachms and thirty-fix grains of chalk are really compofed of one ounce, two drachms, eighteen grains and three quarters of alkaline earth --- three drachms and three quarters of a grain of water, and fix drachms, fixteen grains and a half of elaftic fluid; it muft neceffarily follow that thefe different fubftances combined with each other in the fame proportions, fhould make a calcareous earth or chalk. To obtain a full proof of this, the following experiment was made.

EXPERIMENT.

EXPERIMENT VII.

To reproduce Calcareous Earth or Chalk, by restoring to Lime the Water and Elastic Fluid of which it has been deprived by Calcination.

I weighed five drachms and twenty-two grains of quick lime. We may recollect that this quantity is precifely adequate to one ounce, one drachm and fifty-four grains of chalk. The lime was thrown into eight pints of diftilled water. It was very foon divided by the water, and in part diffolved; but a confiderable portion remained depofited at the bottom of the veffel.

I took, on the other hand, a glafs bottle A. fig. 4. tubulated at E; and filled it up to B.C. fig. 5. i. e. nearly about a third, with chalk in grofs powder. I then fitted the funnel G. to it, which was luted well to the neck of the bottle, fo that there could be no communication of air by by the juncture; and fixed to the end of a ſmall rod O. P. a cork P. of ſuch a ſize as exactly to ſtop the neck of the funnel G. I luted to the orifice E, a glaſs ſyphon E. H. I. and brought its extremity I. to the bottom of a delph bucket K. L. M. N. which contained the lime water. Laſtly, I filled the funnel G. with dilute vitriolic acid, and lifted up, from time to time, the cork P. that ſome portion of the acid might be introduced into the bottle A.

The air which was detached in the efferveſcence, occaſioned by the diſſolution of the chalk in the vitriolic acid, paſſed through the ſyphon E. H. I. and bubbled in the lime-water contained in the veſſel K. L. M. N. At the ſame time the lime-water became turbid, and having continued ſo for a conſiderable time, I proceeded ſo far as to precipitate all the lime, and to render the water which floated above it abſolutely ſweet. I then decanted the liquor; the earth which remained at the bottom was dried in a heat equal to that in which quickſilver boils; after which it was found to weigh one ounce, one drachm and thirty-ſix grains. Its weight, according to the

the foregoing refults, fhould have been one ounce, one drachm and fifty-four grains. This difference of eighteen grains, which cannot be deemed very confiderable, happened either from the inevitable lofs which muft attend almoft every experiment, were it only from a fmall quantity of the earth remaining attached to the veffels; or perhaps the lime in this experiment was not fo perfectly faturated with the elaftic vapour as it might have been.

This calcareous earth moreover differed in no refpect from chalk. It yielded, by diffolving it in the nitrous acid, a quantity of elaftic fluid nearly equal to what chalk affords; the lofs of weight which it fuffered in the procefs was alfo the fame; it no longer feparated the volatile alkali of fal ammoniac in the cold: in a word, it was not in any refpect diftinguifhable from common powdered chalk.

EXPERIMENT VIII.

To determine the Specific Gravity of Lime-Water before and after Precipitation.

Into ſome diſtilled water which was of a temperature of ſeventeen degrees of Reaumur's thermometer, I plunged a ſilver water-gage repreſented figure 6th. This inſtrument is conſtructed on the ſame principles as that deſcribed by Farenheit in the Philoſophical Tranſactions; viz. the ſhaft D. E. inſtead of being graduated in the ſame manner as Boyle's areometer, had only a ſmall mark engraved at E. nearly about the middle. This ſhaft is but three inches long; at the top is a baſon proper to receive the weights; the inſtrument is to be loaded ſo as to ſink it in the fluid to the mark E, by which the ſpecific gravity may be determined. The waterpoiſe is ballaſted at its lower end, viz. at B. C. with tin. Its weight is nine ounces and ſixty-four grains. That it might ſink to the mark E. in diſtilled water,

water, at the temperature of ſeventeen degrees of Reaumur's thermometer, I was obliged to load it with twenty grains and a half. Hence it follows that it diſplaces a volume of diſtilled water of nine ounces, one drachm, twelve grains and a half.

Having withdrawn the water-gage from the diſtilled water, I threw in a good deal more lime than it could diſſolve; it was then filtered, and as ſoon as it was found to be of the temperature mentioned in the laſt experiment, I plunged the water-gage into it; but it could not be made to ſink to the ſame mark without loading it with thirty-two grains. Therefore the weight of the volume of lime-water diſplaced by the waterpoiſe was nine ounces, one drachm and twenty-four grains; which ſettles the relative ſpecific gravity of diſtilled water to lime-water to be as 1000000 to 1002135.

EXPERIMENT IX.

To determine the Specific Gravity of Lime-Water into which the Elastic Fluid discharged by Effervescence has been suffered to pass.

Elastic air detached by the effervefcence of chalk and fpirit of vitriol was made to bubble, as in experiment VI. into faturated lime-water; as foon as the precipitation was completely finifhed, the liquor was fuffered to ftand ftill, after which it was decanted, and the water-gage was caft into it. The weight of the fluid difplaced was found to be nine ounces, one drachm, twelve grains and three quarters, or fenfibly the fame as that of diftilled water; hence it is apparent that the elaftic fluid had precipitated all the lime, and rendered it infoluble in water.

EXPE-

EXPERIMENT X.

To impregnate Water, or any other Fluid, with Fixed Air or Elastic Fluid.

Figure 7th. repreſents the apparatus by which I performed all experiments of this ſort; it does not differ from that of figure 5th. excepting that I have ſubſtituted the bottle I. tubulated at R. inſtead of the bucket K. L. M. N. Some chalk groſsly powdered was placed in the bottle A. as in the ſeventh experiment, and the vitriolic acid was added gradually through the funnel G.*

In proportion as the elaſtic fluid was diſcharged by the efferveſcence, it was obliged to enter the ſyphon

* Dr. Nooth has communicated to the Royal Society, a deſcription of a very convenient apparatus for impregnating water with fixed air, and of the manner of conducting that proceſs. Vide Philoſophical Tranſactions, Vol. LXV. Part I. page 59.

ſyphon E. H. I. to paſs into the bottle I. and to bubble through the diſtilled water or other liquor confined in it. It is neceſſary that all the junctures of the veſſels be exactly luted in this experiment. The orifice R. muſt alſo be ſtopped with a good cork. By this means we keep an atmoſphere of elaſtic fluid, much more condenſed than atmoſpheric air, in the bottle I. and the liquor abſorbs it more readily and in greater abundance than if it were not compreſſed. It is neceſſary to unſtop the tube R. now and then, for fear the veſſels ſhould break, or that the vapours being too much condenſed ſhould eſcape by the junctures; there is always, beſides, a large quantity of elaſtic fluid ſeparated in the effervefcence, which is not capable of being combined with water, and which it is neceſſary to let out occaſionally.

EXPERIMENT XI.

TO COMPARE THE SPECIFIC GRAVITY OF WATER IMPREGNATED WITH ELASTIC FLUID WITH THAT OF DISTILLED WATER.

SOME diftilled water was faturated with elaftic fluid by the method defcribed in the laft experiment. The water had, very fenfibly, an acidulous tafte, and more confiderably fo, in my opinion, than when prepared by Dr. Prieftley's procefs.

HAVING plunged in the water-gage, reprefented in figure 6th. the fluid difplaced was found to weigh nine ounces, one drachm and thirteen grains in a temperature of $19\frac{1}{3}$ degrees. An equal bulk of diftilled water of the fame temperature weighed only nine ounces, one drachm, eleven grains and a quarter. The difference is one grain and three quarters. So that the fpecific gravity of water impregnated with fixed air is to that of diftilled water as 1000332 to 1000000.

The ſame water, having been agitated by pouring it five or ſix times from one veſſel to another, loſt its acidulous taſte; proceeding then to prove the weight of the water in this ſtate, the bulk diſplaced was only nine ounces, one drachm, eleven grains and one third, viz. nearly the ſame as that of diſtilled water.

It is probable, that if the experiment were repeated in cold weather, one might charge the water with a much larger quantity of elaſtic fluid; but I ſhall poſtpone this experiment for a future occaſion.

EXPERIMENT XII.

To precipitate Lime-Water, by adding Water impregnated with Elastic Fluid.

A quantity of the ſame lime-water, the ſpecific gravity of which was determined in experiment VIII. was placed in a receiver, and mixed gradually with ſome water which had been

been impregnated with elaſtic fluid. The lime-water became inſtantly turbid, and the earth precipitated to the bottom of the veſſel: more of the water impregnated with elaſtic fluid was added, till I was aſſured that the precipitation was completed. The liquor was then ſuffered to ſtand awhile, and as ſoon as it was perfectly clear, the water-gage was thrown in; the ſpecific gravity was found to be nearly the ſame as that of diſtilled water. The difference was only about 0,000095; and even this ſmall variation might proceed, from my not having employed preciſely the proportions of lime-water, and water impregnated with elaſtic fluid, neceſſary to make the precipitation perfect. We may eaſily, in fact, conclude, from comparing this experiment with the following, that on exceeding ever ſo little in the quantity of either kind of water, there will remain alike, in each caſe, a portion of earth united with the water. The earth precipitated in this experiment was no longer in a ſtate of quick lime, it effervesced with acids, and did not ſeparate the volatile alkali from ſal ammoniac without the aid of heat. It was become a true chalk.

EXPE-

EXPERIMENT XIII.

To redissolve the Lime after it has been precipitated, by a fresh addition of Water impregnated with Fixed Air.

THE precipitation of lime water, by mixing with it water impregnated with fixed air, affords a ſingular phenomenon; for if, after having precipitated all the lime, as in the laſt experiment, one continue to add ſtill more of the water impregnated with fixed air, all the calcareous earth which had been precipitated will be rediſſolved, and the liquor become perfectly tranſparent.

I shall examine, in a particular chapter, the effects of water thus charged with a ſolution of calcareous earth combined with elaſtic fluid.

Concluſion of this Chapter.

IN reviewing the different experiments of which I have given an account in the preceding chapter,

chapter, we cannot refuse our assent to the following consequences.

First, That there exists in calcareous stones and earths an elastic fluid, a species of air under a fixed form, and that this air, when it has recovered its elasticity, possesses the principal physical properties of air.

Secondly, That a hundred pounds weight of chalk, according to the above proportions, contains about thirty-one pounds, fifteen ounces of this elastic fluid; fifteen pounds, seven ounces of water; and only fifty-two pounds, ten ounces of alkaline earth.

Thirdly, That it is even possible, that the chalk may contain still less alkaline earth, and more elastic fluid, but that hitherto we are not acquainted with any method of depriving it of more, or of carrying its analysis farther.

Fourthly, That alkaline earth may exist in three different states: first, saturated with water and elastic fluid, as in chalk: secondly, depriv-

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ed of its elaſtic fluid, but ſaturated with water, as in ſlaked lime: and, thirdly, deprived both of its elaſtic fluid and water as in quick-lime.

Fifthly, That quick lime, or alkaline earth, deprived both of its water and elaſtic fluid, contains a great quantity of the matter of pure fire, which it has probably acquired during its calcination, and that to this matter is owing the great heat which is obſervable during the extinction of lime, and its diſſolution in acids.

Sixthly, That it is not ſufficient to ſaturate quick lime with water, in order to deprive it of the ſuperabundant quantity of igneous particles; but that they remain after this operation; ſince ſlaked lime communicates a conſiderable degree of heat to the nitrous acid, in which it is diſſolved; a phenomenon which is not produced by calcareous earth or chalk.

Seventhly, That it is by no means this ſuperabundant igneous matter which reduces the alkaline earth into the ſtate of lime, ſince ſlaked lime, when deprived by the ſlaking of a great

part

part of this fire, is, notwithſtanding, no leſs ſoluble in water, ſtill continues to decompoſe ſal ammoniac without the aſſiſtance of heat, and does not communicate a leſs degree of cauſticity, to either the fixed or volatile alkalis. In a word, it is no leſs lime than before it has been ſlaked.

Lastly, That it is ſufficient that we reſtore to lime, by any means whatſoever, the elaſtic fluid of which it has been deprived, to render it mild, inſoluble in water, and capable of effervescing with acids; in ſhort, to re-eſtabliſh it in the ſtate of calcareous earth or chalk *.

CHAP-

* I have only ſpoken, in this chapter, of one ſpecies of calcareous earth, that I might not throw any confuſion on the experiments, and loſe ſight of the principal object. All the pure calcareous earths, which I have had an opportunity of examining, preſent the ſame phenomena as chalk. They are all compoſed of an alkaline earth and water combined with elaſtic fluid in a fixed ſtate; but they, almoſt all, differ in the proportions in which theſe three ſubſtances enter into their compoſition.

Some experiments even lead me to believe, that it is partly from the difference of theſe proportions, that the ſpars

CHAPTER II.

Of the EXISTENCE of an ELASTIC FIXABLE FLUID in the FIXED and VOLATILE ALKALIS, and of the MEANS by which they may be deprived of it.

AFTER having proved the exiſtence of the elaſtic fluid, under a fixed form, in calcareous earths; that the fluid conſtitutes a conſi-

ſpars poſſeſs their diverſity of figure. I have found, for inſtance, that the ſpecies, deſcribed by Valerius, by the name of *Spathum pellucidum flaveſcens*, contained leſs alkaline earth, and more fixed air, than an equal quantity of chalk. The piece which I made my experiments upon, was taken from the lime-ſtone quarries between Chaumont en Baſſigny and Vignory. Two ounces, ſix drachms, thirty-three grains of it, were neceſſary to ſaturate ſix ounces of the above mentioned nitrous acid; whereas, two ounces, conſiderable part of their weight; and that the cauſticity of lime is principally owing to the abſence of it; it remains for me to trace the combination of this fluid with different ſubſtances in nature, and particularly with alkaline ſubſtances and with metals.

The fixed vegetable alkali, which is produced by the burning of vegetables, and commonly known by the name of ſalt of tartar, appeared to

ounces, three drachms, thirty-ſix grains of chalk, were ſufficient for the ſame purpoſe. On the other ſide, the loſs of weight, after the combination, inſtead of being exactly an ounce, as in chalk, was an ounce and two drachms. The ſolution of this ſpar had a greeniſh caſt, and there remained a ſmall white ſediment, inſoluble in acids.

A spar from St. Marie-aux mines, in white cryſtalline groupes, of the Druſen ſpecies, which has much affinity to that repreſented in fig. 7. of Valerius's Mineralogy, afforded nearly the ſame reſults as chalk. The quantity neceſſary to ſaturare ſix ounces of nitrous acid was two ounces three drachms; and the loſs of weight, in the combination, one ounce and three grains. This ſpar depoſited a yellow ſediment, which would not diſſolve in acids. I intend ſometime to purſue theſe experiments farther.

to me not well adapted to be employed in the experiments of which I am going to give an account. 1ſt, Becauſe it is difficult to bring it always to one fixed and determinate point of dryneſs, and the greater or leſs quantity of water which it retains, may occaſion very conſiderable errors. 2dly, That having a very ſtrong propenſity to attract the moiſture from the air, it changes its weight almoſt every moment. The cryſtals of ſoda, or mineral alkali purefied, cryſtallized, and dried on brown paper, appeared preferable; provided that care were taken to keep them always in bottles, well ſtopped, to prevent their effervefcence. I therefore made uſe of this alkali in the following experiments:

EXPERIMENT I.

THE SOLUTION OF CRYSTALS OF SODA IN THE NITROUS ACID.

INTO a long narrow necked matraſs were put ſix ounces of the ſame nitrous acid as that uſed in the firſt experiment, Chap. I. A given weight

weight of the cryſtals of ſoda was alſo diſſolved in a given quantity of diſtilled water, ſix ounces of nitrous acid were gradually ſaturated with this alkaline liquor, and to arrive at that point, it was neceſſary to employ ten ounces, ſix drachms, ſixty-three grains of water, and ſix ounces, two drachms, fifteen grains $\frac{3}{4}$ of the mineral alkali; ſtill the acid was rather predominant. The total of the ingredients, employed in the combination, weighed twenty-three ounces, one drachm, ſix grains $\frac{3}{4}$.

The efferveſcence was briſk, but unattended by heat; after which the ſame ingredients weighed no more than twenty-two ounces, ſixty-two grains $\frac{1}{4}$. The loſs of weight, therefore, was one ounce, $16\frac{1}{2}$ grains.

EXPERIMENT II.

To measure the Quantity of Elastic Fluid, separated from Soda during its Dissolution in the Nitrous Acid.

I MADE ufe, in this experiment, of a fixth part of the quantities employed in the preceding one. I, therefore, placed, in the phial I. fig. 1ft, one ounce of nitrous acid, and in the bafon Q. one ounce, twenty-fix grains $\frac{5}{8}$ of cryftals of foda, diffolved in two ounces of water. The whole was covered with the large Receiver N. N. O. O. and the water being raifed to a fuitable height, and its furface covered with oil, the fwing was fet in motion.

The effervefcence was brifk, and the quantity of elaftic fluid feparated was 135 cubic inches. If then equal quantities had been ufed in this, as in the preceding experiment, there would have been a feparation of 810 cubic inches of elaftic fluid.

The

The barometer, during this experiment, was at twenty-eight inches one line $\frac{1}{2}$, and Mr. Reaumur's ſpirits of wine thermometer at fifteen degrees $\frac{1}{4}$; from whence it may be concluded, after the rules eſtabliſhed by Mr. de Luc, that the atmoſpheric air weighed, at that inſtant, about $\frac{46}{100}$ of a grain to the cubic inch. If, therefore, the elaſtic fluid which was diſengaged had been only pure air, its weight would have been no more than five drachms, twelve grains $\frac{2}{3}$, but the loſs of weight was found to be one ounce, ſixteen grains $\frac{1}{2}$; from whence an exceſs reſults of three drachms, three grains $\frac{5}{6}$. This difference happens, as has been before obſerved, with reſpect to chalk, either becauſe the fluid, diſcharged by the effervеſcence, is heavier than atmoſpheric air, or becauſe it carries up with it ſome watery vapours.

From this experiment it is evident, 1ſt, That more mineral alkali, than chalk, is requiſite to ſaturate a given quantity of nitrous acid; which ſhews, that the ſalt retains much water in its cryſtallization and compoſition. 2dly, That if, on one ſide, the ſoda, in equal weights, contains a much leſs quantity of elaſtic fluid than chalk; on the other hand, it contains a quantity in ſufficiently exact proportion to its quantity of alkaline matter; for we may recollect, that in ſaturating ſix ounces of nitrous acid with chalk, 800 cubic inches of elaſtic fluid were obtained, and the ſeparation of the ſame fluid from ſoda amounted to 810; but thoſe two quantities may be regarded as not ſenſibly different.

We might, perhaps, from hence, ſuppoſe that ſix ounces, two drachms and 15 grains $\frac{3}{4}$ of ſoda; contains an equal weight of alkaline matter to that contained in two ounces, three drachms and thirty-ſix grains of chalk, and might make a tolerably probable calculation of the proportion of water, of elaſtic fluid and alkaline ſubſtance which the ſoda contains; but I acknowledge, at the ſame time, that a greater number of experiments are neceſſary to give a certain degree of evidence to this calculation. According to this mode of reckoning it would follow, that ſix ounces, two drachms and fifteen grains $\frac{3}{4}$ of ſoda contain only one ounce, two drachms and eighteen grains $\frac{3}{4}$ of alkaline matter, one ounce of elaſtic

elaſtic fluid, and three ounces, ſeven drachms and ſixty-nine grains of water; at leaſt this calculation cannot be far from the truth. When theſe quantities are brought to the hundred, the reſult will then be that one hundred weight of ſoda contains ſixty three pounds ten ounces of water, fifteen pounds fifteen ounces of elaſtic fluid, and twenty pounds ſeven ounces of alkaline matter.

EXPERIMENT III.

THE DIMINUTION OF SPECIFIC GRAVITY, IN A SOLUTION OF THE CRYSTALS OF SODA, BY THE ADDITION OF LIME.

TWO ounces of cryſtals of ſoda were diſſolved in fourteen ounces of diſtilled water. The ſilver hydrometer, repreſented in fig. 6. which diſplaces, as we have before ſeen, nine ounces, one drachm, twelve $\frac{1}{2}$ grains of diſtilled water, in a temperature of ſeventeen degrees of Reaumur's thermometer, was then immerſed into the ſolution; the weight of an equal bulk of the ſolution of ſoda, was found to be nine ounces, four drachms, fifty-ſix $\frac{1}{2}$ grains, which gives the proportion between the ſpecific gravity of diſtilled water, and that of the ſolution of ſoda, as 1000000 is to 1049350.

To this ſolution was added, one ounce of lime ſlaked and dried (Exp. III. chap. I.) viz. an alkaline earth ſaturated with water, but deprived of its elaſtic fluid; I ſhook the liquor, for ſome moments, to give the lime time to act upon the ſoda, after which I left it to ſettle. In a ſhort time the lime gained the bottom of the veſſel, and formed there a body; while the liquor which ſwam above it appeared clear and tranſparent. The hydrometer was caſt in; but the fluid, which was now diſplaced, inſtead of weighing, as before, nine ounces, four drachms, fifty-ſix $\frac{1}{2}$ grains, only weighed nine ounces, four drachms, forty $\frac{1}{2}$ grains; which fixes the relative ſpecific gravity of the ſolution to that of diſtilled water, as 1000000 to 1046313.

Another ounce of lime was then added to the ſame ſolution; it was ſhaken up as before, and

and left to ſettle; the weight of the liquor, diſplaced by the hydrometer, was found to be no more than nine ounces, four drachms, twenty-one grains, viz. in the proportion of 1000000 to 1042612.

I STILL added a third ounce of lime; it precipitated more ſlowly, and did not collect into a body, as in the preceding experiments; the ſolution, however, was yet ſenſibly diminiſhed in its ſpecific gravity; the bulk diſplaced by the hydrometer, did not now exceed, in weight, nine ounces, four drachms, fourteen grains, and the proportionate ſpecific gravity between it and diſtilled water was as 1000000 to 1041093.

ON each addition of lime, the alkaline ſolution effervefced leſs with acids; in ſhort, after the third, it did not effervefce at all; the only appearance that could be obſerved, by attending very cloſely, was ſome very ſmall bubbles which roſe on the ſurface of the liquor, or were attached to the ſides of the veſſels in which the precipitation was made. Whatever quantity of lime was added afterwards, no further diminution could be made in the ſpecific gravity of the ſolution, nor could it be brought to the ſtate of not diſcharging any more ſmall bubbles on mixing it with acids.

THIS experiment affords us the proportion of ſlaked lime, requiſite to reduce the ſoda to a cauſtic ſtate: it appears to be three parts of lime to two of the cryſtallized ſoda. This quantity of lime is, indeed, more than is indiſpenſably neceſſary: but it ſeems better, when we wiſh to obtain a lixivium in the higheſt degree of cauſticity, to employ too much than too little. If quick lime be uſed inſtead of ſlaked lime, equal parts will be ſufficient. We have ſeen, in fact, by Experiment III. Chap. I. that ſlaked lime contains ſomething more that $\frac{1}{4}$ of its weight of water.

HOWEVER favourable this experiment may appear to Dr. Black's ſyſtem, it might, nevertheleſs, be explained alſo on that of Mr. Meyer. The partizans of the laſt might ſay, that the diminution of ſpecific gravity, obſerved in the alkaline ſolution, in proportion as the lime was added,

added, far from proving that the lime attracted any thing from the alkali, proved on the contrary, that the lime supplied the latter with some substance of a lighter nature than this solution, and that this was performed in the same manner as is observed relative to water, the specific gravity of which is diminished by the addition of a spirituous liquor, or of any other which weighs less than itself: that it is very probable also, that this matter was nothing but phlogiston; and, lastly, they might add, that the property of phlogiston, of lessening the specific gravity of liquors with which it is combined, is a fact known in chemistry, of which no doubt can remain; for spirit of wine, oils, and many other substances, furnish examples of it.

I SHALL not stop here to discuss these objections, it would lead me into unnecessary arguments; it is to experiment only we should have recourse to determine their merit; and I shall therefore hasten to pursue that method.

EXPE-

EXPERIMENT IV.

The Augmentation in Weight of Lime which has passed through an Alkaline Solution.

FOUR ounces of cryſtals of ſoda were diſſolved in fourteen ounces of diſtilled water; to which were added two ounces of ſlaked and dried lime, and the liquor was agitated for ſome moments; when all the lime was depoſited, I decanted the liquor; the earth which remained at the bottom was waſhed with ſeveral waters, and then dried in a degree of heat in which mercury boils; when afterwards weighed, it was found to be three ounces ſix grains.

It is evident from this experiment, that ſlaked lime attracts from the alkaline ſolution, ſome ſubſtance which it takes poſſeſſion of, and which increaſes its weight about $\frac{1}{3}$. This ſubſtance cannot be water, 1ſt, becauſe the lime was already

ready ſaturated with it. 2dly, That in attracting the water from the alkaline ſolution, it would concentrate the latter, ſo that the ſpecific gravity of the ſolution ſhould be augmented, inſtead of being diminiſhed, as we have ſeen in the preceding experiment. The following experiments will inform us what that matter is which lime attracts from the ſolution of ſoda.

EXPERIMENT V.

To make whatever Portion is desired of the elastic Fluid of Soda, to pass into Lime, and afterwards to demonstrate it in the Lime.

I dissolved one ounce $26\frac{5}{8}$ grains of ſoda in cryſtals, in two ounces of diſtilled water, which according to Experiment II, ought to contain one hundred and thirty five inches of elaſtic fluid; I added to this ſolution two drachms of ſlaked lime, which, by Experiment V. chapter I. ſhould contain about ſix cubic inches of air. The whole quantity

quantity of elaſtic fluid, therefore, employed in this experiment, was one hundred and forty one inches.

If theſe two drachms of lime had actually attracted from the ſoda a portion of the elaſtic fluid which it contained, it muſt neceſſarily follow, firſt, that the ſoda ſhould contain leſs of it than before. Secondly, that the quantity abſtracted from the ſoda ſhould be found united with the lime. To verify this conjecture, I decanted, on one part, the alkaline ſolution ſwimming above the lime, to the very laſt drop; on the other ſide, I carefully waſhed the lime which was at the bottom, and laſtly I ſaturated each of them, ſeparately, with nitrous acid, in the apparatus deſigned to meaſure the quantity of air diſcharged, repreſented in figure 1.

The alkaline ſolution, inſtead of 135 inches, now only yielded 64; the lime, on the contrary, which ſhould have only produced 6 inches, afforded 80, total 144, which was within three inches of the total quantity employed.

EXPE-

EXPERIMENT VI.

THE ſame experiment was repeated, employing the ſame quantities of mineral alkali and of water, only adding four drachms, inſtead of two, of lime. The alkaline ſolution was decanted, and the lime waſhed with a little water; after which I ſubmitted them both ſeparately and ſucceſſively to the apparatus repreſented in figure 1.

THE ſeparation of air from the alkaline lixivium was only 18 cubic inches. The lime, on the contrary, yielded 132; the whole amount, 150 inches; which is equal, again, within about eight inches, to the total quantity of elaſtic fluid employed in this experiment.

FOUR drachms of ſlaked lime are capable, according to the experiments related in the preceding Chapter, of abſorbing more than 200 cubic inches of elaſtic fluid; however it wanted 18 inches of attracting the 135 inches of air which the ſoda contained. This circumſtance proves,

proves, on one ſide, that the laſt portions of elaſtic fluid have a ſtrong adherence to alkaline ſubſtances with which they are united; on the other hand, that lime, after it is combined with a certain portion of elaſtic fluid, no longer acts ſo powerfully, as before, in abſorbing more.

I SHALL now proceed to the phenomena obſervable in the volatile alkali.

EXPERIMENT VII.

THE DISSOLUTION OF CONCRETE VOLATILE ALKALI IN NITROUS ACID.

INTO a ſmall matraſs, with a long neck, were poured ſix ounces of nitrous acid, and a quantity of concrete volatile alkali was gradually added till the acid was thoroughly ſaturated.

A VERY briſk effervefcence enſued, and the quantity of volatile alkali, neceſſary to completely ſaturate the nitrous acid, was 2 ounces, 6 drachms, 36 grains. The total weight, therefore, of the ingredients employed, was, before their mixture, 8 ounces, 6 drachms, 36 grains.

The

The combination being effected, there only were found 7 ounces, 3 drachms, 60 grains; from whence it follows that the loss, during the effervescence, amounted to 1 ounce, 2 drachms, 48 grains.

EXPERIMENT VIII.

To measure the Quantity of Elastic Fluid discharged from a given Quantity of concrete Volatile Alkali.

I employed in this experiment $\frac{1}{4}$ of the ingredients of the preceding one, viz. $1\frac{1}{2}$ ounce of nitrous acid, and 5 drachms 45 grains of concrete volatile alkali. The combination being made in the apparatus described fig. 1st. yielded me $270\frac{1}{2}$ cubic inches of elastic fluid, which being multiplied by 4, makes 1080 cubic inches for the quantity of elastic fluid contained in 2 ounces, 6 drachms, 36 grains of concrete volatile alkali. The height of the barometer, at the time of this operation, was 28 inches, $1\frac{1}{2}$ line, and of the thermometer 19 degrees. The weight, therefore, of a cubic inch of atmospheric air,

was,

was, as Mr. de Luc has determined it, about the $\frac{45}{100}$ of a grain; consequently, if the elastic fluid, separated from concrete volatile alkali, were no heavier than atmospheric air, 1080 cubic inches ought not to weigh above 6 drachms 54 grains; but the loss of weight amounted (Exp. VII.) to 1 ounce, 2 drachms, 48 grains; on which the same reflections may be made as those with respect to chalk and the alkali of soda *.

EXPERIMENT IX.

The Combination of Lime with a Solution of concrete Volatile Alkali.

Eighteen ounces of distilled water, and two ounces of concrete volatile alkali were mixed together in a vessel which was immediately corked closely. The solution produced cold, as is the case with most salts. When the saline liquor

* Vide Exp. II. Chap. I. &c.

quor had recovered the temperature of the laboratory, which was about 17 degrees of Reaumur's thermometer, I immerſed the ſame ſilver hydrometer, which I uſed in the former experiments; the weight of the fluid diſplaced by it, was found to be 9 ounces, 3 drachms, 65$\frac{3}{4}$ grains, ſo that the ſpecific gravity of this ſolution was to that of diſtilled water as 1037440 to 1000000.

I RETURNED this ſolution into a bottle which was well corked; I then added an ounce of lime ſlaked and dried; the veſſel was ſhaken for ſome moments, then left to ſettle, and the liquor being decanted, the hydrometer was again thrown into it: the bulk of fluid diſplaced by the inſtrument, was found to be ſenſibly lighter than before the addition of the lime. Its weight was only 9 ounces, 2 drachms, 59 grains; that is to ſay, the ſpecific gravity of the ſolution now exceeded that of diſtilled water only in the proportion of 1022492 to 1000000. This ſolution, which, before the lime was added, had only a faint odour of the volatile alkali, became now very penetrating.

To

To this ſolution four drachms of freſh lime were added, the weight of the volume of fluid diſplaced, was now found to be reduced to nine ounces, one drachm, and fifty ſeven grains, viz. its ſpecific gravity was to that of diſtilled water in the proportion of 1008446 to 1000000.

Four drachms more of lime, reduced its weight to nine ounces ſixty-nine grains, conſequently the liquour was lighter than that of diſtilled water* in the proportion of 997058 to 1000000.

The ſolution was then extremely penetrating; the vapours were even ſo ſuffocating that it was impoſſible to proceed in the operation to determine the ſpecific gravity, without taking ſome precautions to avoid them.

Four drachms of freſh lime being again added, the liquor appeared to be lighter than diſtilled water in the proportion of 990790 to 1000000.

This

* The ſuperior levity of the fluid volatile alkali to that of water has been already obſerved by Mr. Baumé, relatively to that drawn from ſal ammoniac by means of quicklime. See Chymie experimentale et raiſonnée, p. 112.

This is the point at which the volatile alkali is deprived of its elaſtic fluid as much as it can be by lime; for on adding afterwards four drachms of lime to the alkaline ſolution, no farther diminution was produced in its ſpecific gravity *.

This experiment evinces, that the greateſt quantity of *ſlaked lime*, neceſſary to render the volatile alkali as cauſtic as it can be made by that means, is two parts and half; according to this proportion, rather leſs than two parts of *quick-lime* will be ſufficient to produce the ſame effect; but it is much preferable to uſe *ſlaked lime*; becauſe the great heat which the liquor acquires in the extinction of the lime, would diſſipate a portion of the volatile alkali.

* The total quantity of lime made uſe of in this experiment was exactly three ounces.

EXPERIMENT X.

THE INCREASE OF WEIGHT IN LIME WHICH HAS BEEN COMBINED WITH A SOLUTION OF CONCRETE VOLATILE ALKALI.

To prove, as in Experiment IV. that lime attracts ſomething from the volatile alkali, the ſolution, which had been diminiſhed in its weight, in the laſt experiment, was decanted, and all the lime which had ſettled to the bottom was carefully ſeparated. I dried it, by keeping it expoſed, for a long time, in a ſand bath, to a degree of heat rather greater than that in which mercury boils, and therefore capable of driving off any volatile alkali which might remain interpoſed among its particles; after which having weighed it, I found its weight to be 3 ounces, 4 drachms, 60 grains, whereas it only weighed exactly 3 ounces before the operation.

IF, now, we calculate, after the proportions of of Experiment VIII. we ſhall find that the two ounces of concrete volatile alkali employed in Eperiment IX. ſhould contain 768 cubic inches of elaſtic fluid; but theſe 768 inches of elaſtic fluid, by paſſing into the lime, occaſioned in it an augmentation in weight of 4 drachms 60 grains; each inch, then, of elaſtic fluid weighed $\frac{45}{100}$ of a grain; which is preciſely the ſame as the weight of a cubic inch of atmoſpheric air.

It may be here objected, that I ſuppoſe, in this experiment, that the elaſtic fluid has paſſed from the volatile alkali into the lime, without demonſtrating it. The following experiment will remove this objection.

EXPERIMENT XI.

To demonstrate in the Lime the Quantity of Elastic Fluid which it has attracted from the Volatile Alkali.

Five drachms, 45 grains of concrete volatile alkali were diſſolved in a ſufficient quantity of

 diſtilled

diſtilled water; I then added half its weight, or 2 drachms, 58 grains of ſlaked lime; I agitated the liquor, and when I thought the lime had exerted all its force, the liquor which ſwam above it was decanted, and I, ſeparately, ſubmitted both the lime which was depoſited at the bottom of the veſſel, and alſo the volatile alkali to the ſame apparatus, figure 1ſt. The ſeparation of air afforded by the lime was 163 inches, that furniſhed by the volatile alkali was nearly, as much as it ſhould be to make up the 270 cubic inches of elaſtic fluid contained in five drachms, 45 grains of volatile alkali; I ſay, nearly as much, becauſe one circumſtance of the experiment, which it is inſignificant to repeat, occaſioned an uncertainty of ſome inches as to the product obtained from the volatile alkali.

EXPE-

EXPERIMENT XII.

TO RESTORE TO A CAUSTIC ALKALINE LIXIVIUM OF SODA, THE AIR OF WHICH IT HAS BEEN DEPRIVED BY LIME, AND AT THE SAME TIME TO RESTORE IT TO ITS ORIGINAL SPECIFIC GRAVITY AND THE PROPERTY OF EFFERVESCING WITH ACIDS.

I TOOK the alkaline lixivium of Experiment III. which had been deprived of its air by lime, and placed it in the apparatus reprefented figure 7th, and I caufed the elaftic fluid detached from chalk by the vitriolic acid to bubble into it.

WHEN only a fmall quantity of the cauftic alkaline lixivium was put into the bottle I. it recovered its effervefcent property in three or four minutes; a longer time was neceffary in proportion as the bulk of the liquor was more confiderable; but in each cafe its fpecific gravity was fenfibly augmented, and at the end of the experiment it was nearly the fame as before its combination with the lime.

EXPERIMENT XIII.

To restore to the caustic Volatile Alkali the Air which had been attracted from it by Lime, and at the same Time all the Properties depending on that Air.

The volatile alkali of Experiment IX. of this Chapter, rendered cauſtic by lime, was placed in the bottle I. figure 7th. and the elaſtic fluid diſcharged from chalk by the vitriolic acid was made to paſs through it. The liquor was gradually increaſed in ſpecific gravity; its quick and penetrating odour was rendered mild; and, laſtly, it recovered the property, which it had loſt, of effervefcing with acids, and of precipitating calcareous earth diſſolved in the nitrous acid *.

* This laſt circumſtance is connected with Experiment I. of the following Chapter.

CHAPTER III.

OF THE PRECIPITATION OF CALCAREOUS EARTH, DISSOLVED IN NITROUS ACID BY ALKALIS IN A CAUSTIC, AND IN A MILD STATE.

HAVING made a three-fold combination of nitrous acid and calcareous earth, as alſo the fixed and volatile alkalis, with elaſtic fluid; and having ſhewn how the laſt paſſes from alkalis into calcareous earth, and how it may be driven from the latter by means of acids; I thought it neceſſary, after the examples of Meſſrs. Black and Jacquin, to render theſe combinations more complex, to make them quadruplicate; and I ſhall now relate the phenomena which theſe experiments preſented to me.

I FIRST diſſolved one ounce, five drachms, thirty-ſix grains of ſlaked lime, in ſix ounces of nitrous

nitrous acid *. This ſolution was again divided into four equal parts, and placed in as many ſeparate veſſels; it is evident that each of theſe contained one ounce and a half of nitrous acid, and three drachms, twenty-ſeven grains of ſlaked lime. I kept them to make the four following Experiments.

EXPERIMENT I.

The Precipitation of Lime dissolved in Nitrous Acid by Fossile Alkalis.

TO one of the four portions of the above ſolution, was added, drop by drop, ſome alkali of ſoda in a liquid form, which was continued till no more precipitation was produced; there was neither motion, nor effervefcence in the mixture, and the precipitate collected together in a white form. The liquor which was above it was decanted, the powder waſhed in ſeveral portions of diſtilled water, and, having dried it in the heat of

* See Chapter I. Experiment V.

of boiling mercury, it weighed four drachms, ſixty grains.

THIS earth effervefced briſkly with acids; it had very little taſte, and did not ſeparate the volatile alkali from ſal ammoniac without heat; in a word, it was no longer in the ſtate of lime, but in that of calcareous earth or chalk.

EXPERIMENT II.

THE PRECIPITATION OF CALCAREOUS EARTH DISSOLVED IN NITROUS ACID BY CAUSTIC FOSSILE ALKALI.

INTO the ſecond portion of the ſame ſolution I poured ſome lixivium of alkali of ſoda, which had been deprived of its air by lime*. The precipitation was made in the common manner; having afterwards waſhed and dried the precipitate, its weight was three drachms forty-eight grains. This earth was a true lime; it diſſolved

* See Chapter II. Experiment III.

diffolved in water, in the fame proportion as lime; the lime-water prepared with it, threw up a cremor calcis on its furface; it made fcarcely any effervefcence with acids; it communicated caufticity to alkalis; it decompofed fal ammoniac without heat; in fhort, no difference could be perceived between this and real lime prepared by calcination.

EXPERIMENT III.

The Precipitation of Calcareous Earth, dissolved in Nitrous Acid, by a Solution of Concrete Volatile Alkali.

THE precipitation, in this Experiment, was made with a fufficiently fenfible effervefcent motion, and this circumftance furnifhes alfo a new confirmation of the theory. It has been feen, in fact, Chapter II. Experiment VIII. and Chapter I. Experiment II. that the volatile alkali contains more elaftic fluid than calcareous earth; the latter, therefore, cannot abforb, during its precipitation, the whole of what is difcharged from the

volatile

volatile alkali in its ſolution, and there muſt neceſſarily be an overplus which having recovered its elaſticity, muſt be diſſipated by efferveſcence. The precipitate was of a yellowiſh white; and being dried in the ſame manner as in the former experiments, it weighed 4 drachms, 49 grains. This earth, like that of Experiment I. of this Chapter, was in the ſtate of calcareous earth; it was inſoluble in water; it efferveſced with acids, and had none of the characteriſtics of lime.

EXPERMENT IV.

THE PRECIPITATION OF CALCAREOUS EARTH, DISSOLVED IN NITROUS ACID, BY CAUSTIC VOLATILE ALKALI.

I ATTEMPTED this precipitation in vain, both with the volatile alkali of ſal ammoniac ſeparated by lime, and with concrete volatile alkali deprived of its elaſtic fluid by the addition of lime, as alſo by a volatile alkali ſeparated from ſal ammoniac by metallic ſubſtances and well freed from elaſtic fluid; in all theſe caſes the calcareous

ous earth was not precipitated; I only obferved that, fometimes, the liquor loft fomething of its tranfparency, that a yellow matter, in time, collected, like very fine ruft of iron, which, when dried, weighed but a few grains*.

It appears from thefe four experiments, 1ft, That we may, at pleafure, precipitate an alkaline earth from its folution in nitrous acid, either in the form of chalk, viz. faturated with elaftic fluid, or in the form of lime. It is lime, if it be precipitated by a cauftic alkali, or an alkali deprived of its elaftic fluid: and if precipitated by a mild alkali, it is chalk. 2dly, That when it has been precipitated under the form of lime, its weight is nearly the fame as that of the original lime employed in the folution, whereas, on the contrary, when it is precipitated under the form of calcareous earth or chalk, i. e. faturated with elaftic fluid, it is obtained with an increafe of weight nearly equal to that which lime, converted

* It has been already feen, Chapter II. Experiment XIII. that by reftoring the elaftic fluid to the cauftic volatile alkali, the property of precipitating calcareous earth was alfo reftored to it.

verted into chalk, acquires. 3dly, That ſomething was however wanting to render this augmentation as great as it ſhould be; for it appears from the experiments related at the beginning of Chapter I. that 3 drachms, 27 grains of ſlaked lime, afterwards ſaturated with elaſtic fluid, ought to weigh 4 drachms, 63 grains. We had, however, by the foſſile alkali, Experiment I. but 4 drachms, 60 grains, and by the concrete volatile alkali, Experiment III. only 4 drachms, 49 grains; which farther confirms, what was above advanced, that lime, which very powerfully attracts the firſt portions of elaſtic fluid which are preſented to it, acts but weakly on the laſt.

The Experiments related in theſe two chapters, give almoſt the ſtrongeſt proofs that can be adduced in phyſics, that the ſame elaſtic fluid, which has been ſeen to exiſt in chalk, Chapter I. is alſo found in the fixed and in the volatile alkalis; that it may be driven off from them by diſſolving them in acids; and that the effervеſcence, obſervable at the inſtant of their combination, is cauſed by the ſeparation of this fluid. That the ſame

ſame fluid has alſo more relation, more affinity with lime, than with alkaline ſalts; and that it is for this reaſon, that if we mix lime with an alkaline ley, it attracts the elaſtic fluid which the alkali contained, unites with it, is converted into calcareous earth, and reduces the alkali to a cauſtic ſtate.

This might, perhaps, be the time to relate the experiments which I have made on the nature of elaſtic fluid ſeparated from alkaline ſalts and earths; but other conſiderations oblige me, firſt, to inquire into the combination of this fluid with metallic ſubſtances.

CHAP.

CHAPTER IV.

Of the COMBINATION of the ELASTIC FLUID of CALCAREOUS EARTH and ALKALIS with METALLIC SUBSTANCES by PRECIPITATION.

EXPERIMENTS, ſufficiently numerous, lead me to believe, that the elaſtic fluid, the ſame whoſe exiſtence I have endeavoured to prove in calcareous earth and alkalis, is capable of uniting, by precipitation, with moſt metallic ſubſtances; that it is, in a great meaſure, this principle which forms the augmentation of weight in metallic calces, which deprives them of their brilliancy, which reduces them to the form of a calx, &c. Though my experiments on this ſubject are very numerous, yet as it is not to be doubted, that the precipitates retain with them ſomething, both of their ſolvents and of the ſubſtances which have been uſed to precipitate them; and that to this circumſtance, alſo, ſome particular phenomena are joined, which are cauſed by the decompoſition of the acids; I thought it better to reſerve, for a particular memoir, the greater part of my experiments; and ſhall, therefore, content myſelf with delivering, at preſent, thoſe which are more eſſentially connected with the ſubject which I am now treating; but with this caution, however, to the reader, that they are related but as facts, the conſequences of which are not yet ſufficiently proved.

EXPERIMENT I.

THE SOLUTION OF QUICK-SILVER IN THE NITROUS ACID.

EXACTLY twelve ounces of quick-ſilver were put into a matraſs, and twelve ounces of ſpirit of nitre* poured on it; immediately a ſpontaneous

* See Chapter I. Experiment I.

ſpontaneous effervefcence enſued attended with heat. The red vapours of the nitrous acid aroſe from the mixture, and the liquor aſſumed a greeniſh colour. I did not wait till the ſolution was entirely accompliſhed, before I weighed it; it had loſt one drachm, eighteen grains: three hours after, the mercury was nearly all diſſolved; but having again weighed the ſolution, I was much aſtoniſhed to perceive, that it had increaſed, inſtead of being diminiſhed in weight, and that the loſs, which was one drachm eighteen grains, at firſt, was now only fifty-four grains. The next day the ſolution of the mercury was entirely finiſhed, and the loſs of weight reduced to eighteen grains; ſo that in twelve hours, the ſolution, though confined in a narrow necked matraſs, had acquired an augmentation in weight of one drachm. Not having leiſure at that time to purſue this phenomenon farther, I poſtponed my inquiry to a future opportunity; I added ſome diſtilled water to my ſolution, to prevent it from cryſtallizing, the total weight of it was then found to be forty-eight ounces, one drachm, eighteen grains.

EXPERIMENT II.

The Precipitation of Quick-silver by Chalk and by Lime.

I WEIGHED feparately, in two veffels, eight ounces, fifteen grains of the above folution, each of which portions, according to the preceding experiment, ought to contain two ounces of nitrous acid, and two ounces of quick-filver. On the other fide, I prepared fix drachms, thirty-fix grains of chalk, and four drachms, thirty-fix grains of flaked lime. It has been feen, Chapter I. Experiments I. and IV. that thefe were the proportions neceffary to faturate two ounces of nitrous acid. I put the chalk into one of the veffels, and the lime into the other.

An effervefcence attended the precipitation by the chalk, but without any heat; the mercury precipitated in a light yellow powder; at the fame time the chalk was diffolved in the nitrous acid.

The

The precipitation by the lime was effected without effervefcence, but with heat; the mercury was precipitated in a brownifh powder. When the precipitates were well fubfided, I decanted off the liquor from them, and carefully edulcorated them. After which I caufed them to be dried in a heat nearly equal to that in which mercury boils.

The precipitate by the chalk weighed two ounces, two drachms, forty-five grains; that by the lime weighed two ounces, one drachm, forty-five grains. It was of a deep grey earthy appearance.

EXPERIMENT III.

The Solution of Iron by the Nitrous Acid.

SIXTEEN ounces of nitrous acid, the fame as employed in the preceding Experiments, were placed in a matrafs, and fome iron filings gradually added; the effervefcence was brifk, attended with great heat, red vapours, and a very rapid difcharge of elaftic fluid: the quantity of iron, neceffary to attain the point of faturation, was two ounces four drachms; after which the lofs of weight was found to be four drachms nineteen grains. As the folution was turbid, I added as much diftilled water as made the whole weight of the folution to be exactly fix pounds.

EXPERIMENT IV.

The Precipitation of Iron, dissolved in Nitrous Acid, by Chalk and by Lime.

I TOOK two portions, each weighing twelve ounces of the above folution, and containing two ounces of nitrous acid, and two drachms, thirty-fix grains of iron filings. I placed them in two feparate veffels; to one were added fix drachms, thirty-fix grains of chalk, and to the other four drachms, thirty-fix grains of flaked lime, being the quantities neceffary to faturate the acid.

The precipitation was effected by the chalk with effervefcence and tumefaction; that by the lime,

lime, without either effervefcence or heat. Each precipitate was a yellow-brown ruft of iron. They were wafhed in feveral parcels of diftilled water, and then dried in a fand heat fomething fuperior to that ufed in the laft experiment.

THE precipitate by the chalk when dried, was a greyifh ruft of iron, inclining even to white by veins; it weighed 6 drachms, 35 grains; that by the lime was rather yellower, and weighed 4 drachms, 69 grains.

THE refults of thefe experiments are, 1ft, That iron and mercury diffolved in the nitrous acid, acquire in general a remarkable increafe of weight, whether they be precipitated by chalk or by lime. 2dly, That this increafe is greater in refpect to iron than to mercury. 3dly, That one reafon for thinking that the elaftic fluid contributes to this augmentation is, that it is conftantly greater when an earth is employed faturated with elaftic fluid, fuch as chalk, than when an earth is ufed which has been deprived of it, as lime. 4thly, That it is probable that the increafe of weight which is experienced in the precipitation by lime, although not ſo great as that by chalk, proceeds, in part, from a portion of elaſtic fluid which probably remains united to the lime, and which could not be ſeparated by the calcination. Experiment VI. Chapter I. confirms this opinion; we there actually ſee, that ſlaked lime ſtill contains ſome portion of elaſtic fluid.

To theſe experiments, which ſeem to lead to a belief that the augmentation of weight in the metallic precipitates is partly due to a portion of elaſtic fluid which is united to them, one very ſtrong argument may be joined; which is, that if, inſtead of precipitating by an earth, the precipitation be made by another metal, as ſhewn in columns 2 and 3 of Geoffroy's table of affinities, the diſſolved metal, inſtead of being precipitated in form of a calx, reappears, on the contrary, in a metallic form, and is no heavier than it was, previous to its ſolution: It is very probable that this circumſtance depends on the metal not finding, in its precipitation, any body from which it can attract the elaſtic fluid.

I SHALL

I SHALL employ myfelf more particularly on this fubject, at fome future period.

CHAPTER V.

ON THE EXISTENCE OF ELASTIC FIXABLE FLUID, IN THE METALLIC CALCES.

ALLOWING that the experiments, related in the laft chapter, may not have completely proved the poffibility of the union of elaftic fluid with metallic fubftances, they at leaft afforded fuch indications of it, as were fufficient to engage me to purfue the fubject with particular attention. I began, from this time, to fufpect, that the air of the atmofphere, or an elaftic fluid contained in the air, was capable, in a great many circumftances, of being fixed and combined with metals; that to the addition of this fubftance, the phenomena of calcination were

were owing, as likewife the augmentation in weight of the metals converted into calces, and, perhaps, feveral other phenomena of which philofophers have, as yet, given us no fatisfactory explanation. Thefe conjectures alfo acquired a very great degree of probability, in my opinion, from the following reflections.

1ft. The calcination of metals cannot take place, in veffels clofely ftopped and exhaufted of air.

2dly. It is proportionably the more readily performed, as a greater extent of furface in the metal is expofed to the air.

3dly. It is a fact known to all the metallurgifts, and obferved by all thofe who have been converfant in the operations of affaying metals, that in every reduction, there is an effervefcence at the moment the metallic fubftance paffes from the ftate of a calx to that of a metal; now, an effervefcence is commonly no more than a feparation of elaftic fluid; the calx therefore contains an elaftic fluid, under a fixed form, which recovers

recovers its elafticity, at the inftant of reduction.

HOWEVER probable thefe conjectures might appear to me, it was by experiment, alone, they could be either confirmed or refuted ; I accordingly made a fucceffion of different trials, feveral of which were not fuccefsful, and the detail of which I think I ought to fpare the reader, till I finally attain the eftablifhment of the following facts.

EXPERIMENT I.

THE REDUCTION OF MINIUM IN AN APPARATUS PROPER TO MEASURE THE QUANTITY OF ELASTIC FLUID DISCHARGED OR ABSORBED.

B. C. D. E. fig. 8th. reprefents a ciftern or other veffel of delf or glafs, into which is inverted a cryftal receiver F. G. H: in the middle of the ciftern at K. a fmall column of cryftal I. K. is erected

erected formed into a cup at the top, and which is to be ſecured at the bottom with ſome green wax *. On this column is placed a cupel of porcelain or any other very refractory matter. A glaſs ſyphon or bent tube M. N. fig. 9th. is to be paſſed under the edges of the receiver, and the ciſtern B. C. D. E. is to be filled with water. The water then is made to riſe to ſuch a height as is judged neceſſary, in the receiver F. G. H. by ſucking the air through the aperture N. of the ſyphon M. N. and then, with the funnel, with a curved neck, repreſented fig. 3d. a covering of oil is introduced into the receiver: the oil riſes to the ſurface and prevents the elaſtic fluid, which is ſeparated in the operation, from having immediate contact with the water or being abſorbed by it.

In the capſule A fig. 8th, were placed two drachms of minium, mixed with twelve grains of baker's fuel, which had been previouſly reduced

* These kind of columns may be procured at moſt of the china ſhops; they are uſed in deſerts to ſupport the fruit.

to

to powder, and calcined for ſeveral hours, by a ſtrong fire in a cloſe veſſel. The height, to which the water, had been raiſed, G H, was marked with a narrow ſlip of paper, and the apparatus, thus diſpoſed was carried to the focus of one of Tchirnauſen's large burning glaſſes, belonging to M. le Comte de la Tour d' Auvergne: this lens was at that time placed at the Louvre, for the purpoſe of other experiments made by Meſſrs. Macquer, Briſſon, Cadet, and myſelf jointly, of which one part is already communicated to the Academy of Sciences.

Almost at the very inſtant that the cupel A was preſented to the focus, the reduction was accompliſhed and the lead reappeared in little round pieces or very ſmall ſhot: at the ſame time a yellowiſh vapour aroſe, which adhered to the arch of the receiver, and ſeemed, to me, to be a calx of lead which had been volatilized by the violent heat. When I thought the reduction was finiſhed, I withdrew the apparatus from the focus, and placed it on the ſame ſtand, and exactly in the ſame place in which it was before the operation; and when the veſſels were perfectly cooled,

cooled, and had recovered the temperature which they had previous to the reduction, I obſerved the height of the water, and diſcovered, by the ſinking of the ſurface, that a ſeparation had been made of about fourteen cubic inches of elaſtic fluid.

The quantity of lead obtained by this reduction was about $\frac{1}{32}$ of a cubic inch, whence it follows that the volume of elaſtic fluid ſeparated, was equal to four hundred and forty eight times the volume of the lead reduced; there were ſtill found at the bottom of the cupel ſome portions of minium unreduced. This experiment was frequently repeated and with different proportions; thoſe which I have here mentioned conſtantly ſucceeded beſt: when too much charcoal was uſed, the reduction was made with difficulty at the bottom of the veſſel; the charcoal on the contrary was burned on its ſurface, and there reſulted errors ſo conſiderable as to prevent any confidence in the events.

Though this experiment was ſufficiently deciſive, I ſtill remained diſſatisfied; firſt, becauſe the

the focus of the burning glaſs being very narrow. I was not able to operate but on ſmall quantities. Secondly, becauſe the heat was ſo great in the vicinity of the focus, that it was impoſſible for me to employ receivers of leſs than five or ſix inches in diameter; and even theſe were much heated, and ſome of them were broken; from hence it happened that the ſmall number of cubic inches ſeparated during the reduction, being divided in a ſpace ſo extended in ſurface, the differences were little perceivable. Thirdly, becauſe the volume of air, contained in the receiver, being very conſiderable, the leaſt difference in the temperature might occaſion ſenſible errors. And fourthly, becauſe the oil alſo, which covered the ſurface of the water, being expoſed to ſo conſiderable a degree of heat, might diſcharge ſome portions of elaſtic fluid.

THESE different conſiderations obliged me to have recourſe to an apparatus repreſented in fig. 10. the firſt idea of which came from Dr. Hales; it has been ſince corrected by the late Mr. Rouelle, and I have alſo made ſome occaſional additions and alterations in it.

THE

The retort A. fig. 10. is fitted at G. G. to the receiver G. H. which may be, according to the nature of the operation, either of tin, plated iron, or glafs; the receiver has an aperture at *h.* which is lengthened in a tube *h.* I. about $2\frac{1}{2}$ feet in length. V. V. F. F. is a large bucket of wood, or rather of metal, perforated at K. K. in which the receiver G. H. is placed; and all the parts are fecured with either maftick or folder, according as it may be glafs or metal. Laftly, the whole is covered with a large glafs receiver *n.* N. O. O. which fhould be perforated with a fmall hole at *n.* This receiver is fupported by a pedeftal, compofed of four fmall pillars, kept at a convenient diftance, by means of metal bands. Thefe pillars are notched at the top, to receive the edges of the receiver.

When we ufe this apparatus, the fubftances on which we intend to operate are placed in the retort A. It is luted clofely at G. G. to the receiver G. H. with fat lute, of rather a thick confiftence. This operation ought to be conducted with the greateft attention, and the lute muft by no means be omitted, as it is extremely effential, that

that not the leaſt particle of air ſhould be introduced through the junctures. The lute is to be covered with a moiſtened bladder, which is to be ſecured by thread paſſed ſeveral times round, pretty tight. It is not unuſeful to remark, that before the thread be paſſed over the lute, it is neceſſary that the bladder be firſt firmly tied both above and below the juncture; to prevent the lute from ſpreading further than is neceſſary, and eſcaping from the preſſure of the thread.

When the veſſels are thus luted, the ciſtern V.V.F.F. is to be filled with water, which is to be pumped up into the receiver by ſuction at the hole *n*. and raiſed to whatever height is deſired; care muſt be taken to fill up the ciſtern in proportion.

The ſuction is not ſo eaſily performed as may be imagined; it becomes even extremely troubleſome when the height of the water approaches 28 or 30 inches. This difficulty appeared to me ſo important, that it was neceſſary to remedy it; and I accompliſhed it by applying to this apparatus the little pump repreſented fig. 1. I introduced under the recipient *n.* N. *o. o.* fig. 10. a tin ſyphon or tube E. B. C. D. repreſented ſeparately fig. 11. Its extremity D. is proportioned ſo as exactly to fit it into the tube S. S. fig. 10. which is furniſhed with a cock R. and whoſe other end is adapted at S. X. to the tube X. L. of the pump P. When the junctures D. S. and S. X. have been exactly ſecured with the fat lute or green wax covered with a hog's bladder, moiſtened and tied round with ſtrong thread, the cock R. is to be opened, the piſton Z. put in action, and the air, contained in the receiver *n.* N. *o. o.* pumped out, and thus we are able to raiſe the water conveniently to the neceſſary height.

The operations performed, with the aſſiſtance of this apparatus, were on the calx of lead, the reduction of which is ſo eaſy that I did not foreſee that any difficulty would occur in the execution of it. I had very much, however, to encounter, from my embarraſsment in the choice of retorts; thoſe of glaſs are ſo liable to be acted on by the calxes of lead, that they loſe their form, and flux before the reduction be made. Thoſe of earth reſiſt them better, but almoſt all

of

of them have fmall imperceptible holes through which the air penetrates, fo that one can fcarcely ever be eafy about the fuccefs of the experiments.

THESE difficulties interrupted me for a long time; and it was not till I was able to procure iron retorts that I began to operate commodioufly. As the fame obftacles which have obftructed me, may alfo occur to thofe who would repeat my operations; I fhall give fome defcription of the fabrication of the retorts which I ufed.

A PIECE of the ftrongeft iron plate that can be procured is to be forged into the fhape of a cap A. A. B. fig. 12. to form the bottom of the retort; three parcels are afterwards to be made from the fame plate A. A. C. C, C. C. D. D, D. D. E. the edges of which muft fit into each other very exactly; the lateral juncture of each ferrel is to be carefully foldered with copper, and the ferrels are to be united to each other and to the cap A. A. B. with the fame folder. The only difficulty in this matter is with the folder which is reſerved for the laſt, as it muſt be done on the outſide, but a dextrous workman will eaſily accompliſh it, and I have not been much diſappointed in this reſpect. Theſe retorts may be made ſufficiently red-hot without melting the ſolder; only this precaution is neceſſary, that when we uſe metallic bodies which are capable of attacking the copper, and of uniting with it, we fill only the lower part A. A. B. of the retort, below the ſolder. The ſame retort may be uſed frequently, and needs not be laid aſide till the iron be burnt and reduced to ſcales. However attentive the workman may be, yet it is poſſible that ſome ſmall imperceptible holes may remain in the ſolder, through which the air may be introduced; the method of diſcovering them is to pour a ſmall quantity of water into the retort, and to ſhake it about till the inner ſurface be totally moiſtened, on blowing through the aperture E. the hole, if there be one, is announced by a ſmall bubble of water which is perceived and points out the flaw.

However tedious theſe preliminaries may ſeem, they will be eaſily judged indiſpenſibly neceſſary for

for underftanding the following experiments; and I chofe to begin with them that I might lefs interrupt the attention of my reader.

EXPERIMENT II.

To reduce Lead by the Fire of Furnaces, in an Apparatus proper for measuring the quantity of Elastic Fluid separated.

IN the iron retort A. fig. 10th. were placed fix ounces of minium, and fix drachms of powdered charcoal, paffed through a hair fieve. It will foon be feen that this quantity of charcoal is much more than fufficient to produce the reduction; but one circumftance makes this proportion neceffary when iron retorts are ufed; for, then, the lead, after its reduction, remains in fmall fhot which are mixed with the powdered charcoal, and are eafily taken out of the retort; whereas, on the contrary, when only juft the neceffary quantity of charcoal is ufed, the lead forms into a mafs, and if it be melted again, in order to get it out, there is danger that ſome part of it may unite with the ſolder, or ſome little of it may adhere to the retort. Theſe inconveniences are avoided by uſing an over proportion of charcoal.

I LUTED accurately, in the manner above related, the retort A. to the receiver G. H. The water was raiſed to Y. Y. and a covering of oil was introduced on the ſurface of it. When every thing was thus diſpoſed, I left the apparatus in the ſame ſtate till the following day that I might be ſure the air did not penetrate on either ſide. I then marked the length of the water at Y. Y. with a ſlip of paper, and lighted ſome charcoal in my furnace.

IN proportion as the veſſels were heated, the air which they contained was rarefied, and the water deſcended accordingly; but this effect had its limits, and after ſome time, the rarefaction abated, and the water continued a while nearly ſtationary. When the fire came to be ſo much raiſed as to make the bottom of the retort red-hot; the water began ſuddenly to deſcend, al-

moſt

moſt viſibly, at the rate of 14 or 15 cubic inches in the minute; at length the ſeparation abated, and when it was entirely ceaſed, I put out the fire and ſuffered the veſſels to cool perfectly. The air contained in the receiver *n*. N. *o*. *o*. preſently condenſed as it cooled, and the water roſe again. When it was quite fixed, I marked, with a ſlip of paper the place at which it ſtopped; and I left the veſſels in the ſame ſtate for 48 hours, without any ſenſible difference enſuing in the height of the water; the thermometer in the laboratory was, at this time, at 15 degrees and the barometer at 28 inches 1½ line.

Nothing now remained but to determine the quantity of cubic inches contained between the two ſlips of paper, and this I did in two different ways. 1ſt. By determining from exact meaſure and calculation the ſolid contents of the cylinder. 2dly. By filling the intermediate ſpace, between the two ſlips of paper, with water, and then determining the weight and volume of this water. The two methods afforded me exactly the ſame reſults, and the quantity of elaſtic fluid

 ſeperated,

ſeparated, was found, from both, to be 560 cubic inches. The quantity of lead reſulting from this reduction was about $\frac{3}{4}$ of a cubic inch; whence it follows, that calx of lead contains a quantity of elaſtic fluid 747 times the bulk of the metal which was uſed to form it. When the veſſels were unluted, I ſhook the retort, and poured out the lead; it was in grains, mixed with a conſiderable quantity of powdered charcoal. Having examined it carefully, I could not find any portion of the minium unreduced. The weight of the reſiduum was 5 ounces 7 drachms 66 grains. The experiment was frequently repeated, and the circumſtances were always exactly the ſame.

The weight of the ingredients employed in this experiment was, before the reduction, ſix ounces ſix drachms; after that proceſs it was no more than five ounces, ſeven drachms and ſixty ſix grains; and therefore the loſs of weight was ſix drachms and ſix grains: but the quantity of elaſtic vapour ſeparated was only five hundred and ſixty cubic inches, and

and an equal bulk of atmofpheric air fhould only have weighed, that day, three drachms and forty one grains. It is true that there is every reafon to believe that the elaftic fluid from metallic reductions, which is the fame as that from effervefcence, as I fhall demonftrate in the fequel, is heavier than atmofpheric air: it has alfo been feen (Chapter I.) that its gravity may be eftimated at $\frac{575}{1000}$ the cubic inch; but even from this reckoning, 560 cubic inches of elaftic fluid fhould only weigh four drachms, thirty-four grains, and there would ftill be a deficiency in weight of one drachm, forty-four grains.

Some drops of phlegm which I conftantly found in the receiver GH, fig. 10th. in all the reductions of the calces of lead which I have made, induced me to fufpect, that, independant of the elaftic fixable fluid, there exifted a portion of water in the minium; that it was feparated during the reduction, and that this was probably the caufe of the lofs of weight which I had obferved; but as the receiver GH, fig. 10th. was too fmall to condenfe the vapours fufficiently, it was thought proper to repeat the experiment with a common apparatus for diſtillation, employing a larger receiver.

EXPERIMENT III.

To determine the Quantity of Water which is separated in the Reduction of Minium by powdered Charcoal.

THE ſame quantities of minium and charcoal were uſed in this experiment as in the laſt: the receiver had a ſmall hole drilled into it, which I was obliged to leave open during the operation. The elaſtic fluid was ſeparated with a hiſſing noiſe, and at the beginning of the reduction ſome little water paſſed into the receiver. The weight of this water did not exceed twenty-four grains; it was an inſipid phlegm, not ſeeming to differ from diſtilled water.

Though the reſult of this experiment only yielded twenty-four grains of phlegm, it is however

however probable that more was difcharged, and that one part of it was carried off by the current of elaftic fluid, and diffipated in vapours through the perforation in the receiver. Again it is probable that the elaftic fluid difengaged from minium may be fomething heavier than that from effervefcence, and it is very probable that to one of thefe two caufes we may attribute the deficiency in weight remarked in Experiment II.

I PROPOSED, in order to clear up this point, immediately to determine the relative gravity of the different elaftic fluids difcharged from different fubftances, and to compare them with the air of the atmofphere: but the different apparatuffes, neceffary to complete this object, not having been procured in time, I thought it would not be right to defer, on that account, the publication of this work; befides, I fhall have occafion, more than once, to recur to this point.

THE quantity of powdered charcoal, employed in Experiment II. was fix drachms; the quantity of elaftic fluid obtained in the reduction did

did not exceed four drachms, or four and a half at the moſt. The weight of the elaſtic fluid ſeparated was then much leſs than that of the charcoal employed; and it may be objected that the quantity of elaſtic fluid, which was diſcharged, might as well come from the charcoal as from the metallic calx. To obviate this objection, I made the following experiment.

EXPERIMENT IV.

To separate from the Lead the Portion of Charcoal which remains after the Reduction.

THE reſiduum of Experiment II. was placed in an iron ladle; we may recollect that it was compoſed of granulated lead and powdered charcoal, and its weight was five ounces, ſeven drachms, ſixty-ſix grains. As ſoon as the charcoal powder began to heat, it lighted and gradually conſumed, after which there only remained a lump of lead, and a little calx of the ſame metal which had formed anew during the burn-

ing

ing of the charcoal. The whole of the lead together weighed nearly five ounces, three drachms, twelve grains; I ſay nearly, becauſe if the operation be not quite finiſhed, a ſmall portion of charcoal remains unburned; on the other hand again, if it be carried too far, one part of the lead is recalcined and augments the weight. This circumſtance occaſions an uncertainty in the reſult of about twelve grains; and it is by repeating the experiment ſeveral times, and adhering to the leſſer weight, that I have fixed it ſuch as it is here.

It appears from this experiment, 1ſt, That the relative weight of lead to that of minium is five ounces, three drachms, twelve grains, to ſix ounces, viz. that with 100 pounds of lead, 111 pounds 10 ounces of minium may be made, or, which is ſtill the ſame thing, that 100 pounds of minium contain 89 pounds 9 ounces of lead. 2dly, That the 5 ounces 7 drachms 66 grains, remaining in the retort, Experiment II. after the reduction, was compoſed of 5 ounces 3 drachms 12 grains of lead, and of 4 drachms 54 grains of charcoal. So that only 1 drachm 18 grains

of

of charcoal, had been actually employed in the reduction: but the quantity of elastic fluid discharged in Experiment II. placing it at the lowest, weighed at least $3\frac{1}{2}$ drachms; it could not therefore be supplied by $1\frac{1}{4}$ drachm of charcoal, and consequently it must necessarily be at the expence of the minium, that the greater part of the elastic fluid was furnished.

Notwithstanding the conclusiveness of this experiment, I was not yet satisfied, and I thought it requisite to attend particularly to the examination whether charcoal alone would not yield, in the same degree of heat, an elastic fluid similar to that which I had obtained in the reduction of minium. This is the object of the following experiment.

EXPE-

EXPERIMENT V.

To calcine powdered Charcoal alone, in a strong Fire, and in an Apparatus proper for measuring the Quantity of Elastic Fluid separated from it.

I CAUSED a new gun-barrel, well cleaned within, to be bent, and the touch hole and breech ſtopped, and each covered with a piece of iron, ſoldered hot to it, that I might be certain that the external air had no admiſſion into it. Two drachms of the ſame bake-houſe fuel powdered (Experiments I. and II.) were then introduced, and it was adapted to the apparatus of fig. 10th. in which, on this occaſion, I was obliged to make ſome trifling changes, which it is unneceſſary to relate: I then luted all the junctures, very exactly, in the uſual manner; the water in the receiver *n*NOO was raiſed and covered with a thin bed of oil; and when I was certain that the air could not penetrate either way, I marked the height of the water y y. I then

then made a brifk fire round the gun-barrel, and kept it for an hour in a white heat.

THE air was rarefied at firft as ufual, and the furface of the water funk proportionably; but after the fire was extinguifhed it reafcended by degrees, and when the gun-barrel was quite cold, it returned nearly to the point from which it began to move. The product of air was only thirteen cubic inches, which in two days was reduced to eight inches. The powdered charcoal being weighed, at the conclufion of this experiment, had loft but fix grains, and it is even probable, that fome of it ftill adhered to the gun-barrel.

THE fire, in this procefs, was infinitely ftronger, and continued for a much longer time than is neceffary for the reduction of the calx of lead, yet the product of air was very fmall; and therefore the air obtained in Experiments I. and II. was not merely the effect of the calcination of the charcoal, but, on the contrary, was produced by the reduction.

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I have before obſerved, that I had made uſe of a new gun-barrel which had been well cleaned on the inſide, in this experiment, and this circumſtance is worthy of remark, for the phenomena are very different, when a gun-barrel is employed which is ruſty within. In that caſe we procure a little water, and a greater production of elaſtic fluid in proportion to the ruſtineſs of the barrel; but it is evident from the laſt experiment, that theſe products belong to the calx of the iron which is reduced, not to the charcoal. It has ſometimes happened that I have obtained from 80 to 100 cubic inches of elaſtic fluid with a very ruſty gun-barrel, the firſt time it has been uſed, I here only mention this experiment, reſerving the different accounts relative to it to a future opportunity.

It might perhaps be ſuſpected, that the gun-barrel which I employed in Experiment V. though new and well cleaned, might yet contain ſome ruſt, and that the ſeparation of the eight inches of elaſtic fluid, which I obſerved, might be owing to this circumſtance; but I was convinced to the contrary, by repeating this experi-

 ment

ment with the ſame barrel and freſh charcoal. It is plain, that if the elaſtic fluid had been produced, in the former experiment, by the reduction of the iron of the barrel, that ſeparation could not take place in the latter. However, the quantity of elaſtic fluid, this laſt time, proved to be twelve inches at leaſt, which was ſomething greater than at firſt; from whence it ſeems demonſtrated that the ſeparation is to be placed to the account of the charcoal.

THE diminution of weight, in this experiment, was eight grains.

EXPERIMENT VI.

THE REDUCTION OF MINIUM IN A GUN-BARREL.

FOUR ounces of minium were mixed with the ſame charcoal which was ſo ſtrongly calcined in the preceding experiment, and the whole put into the ſame gun-barrel which had been uſed in the two former calcinations. It was then adapted

to

to the apparatus, fig. 10th. and all was difpofed in the fame manner as in Experiment V. after which the fire was kindled.

When the gun-barrel began to be obfcurely red, the difcharge of elaftic fluid was fo very rapid, that the water defcended, perceptibly, in the receiver *n*NOO. fig. 10th. The difcharge being finifhed, I continued to raife the fire, but the water did not fall fenfibly lower. When the veffels were grown cold, I meafured the quantity of elaftic fluid which was feparated, and found it to be 360 inches, or in the proportion of 90 inches for each ounce of minium. It has been feen above, Experiment III. that fix ounces of minium had yielded a difcharge of elaftic fluid of 560 cubic inches, which is fomething more than 93 inches for each ounce; from whence it is evident that the refults of thefe two experiments agree almoft perfectly. As in the operation of which I have here given an account, the charcoal was ftrongly calcined a fecond time, before it was mixed with the minium, the refults of this experiment fhould feem to deferve more reliance than thofe of Experiment III.

It appears to be proved from thefe experiments, that it is by no means the charcoal alone that produces the difcharge of elaftic fluid, obferved in Experiments I. and II. neither is it the minium alone, fince after Dr. Hales's experiments (fee page 25) it affords but a very fmall portion of air; the greater part of elaftic fluid which is detached, arifes from the union of the powdered charcoal with the minium. This laft obfervation leads us infenfibly to very important obfervations on the ufe of charcoal, and fuch kind of fubftances in general, in the reduction of metals. Do they ferve, as the difciples of Mr. Stalh think, to reftore to the metal the phlogifton which it has loft? Or rather do thefe fubftances enter into the compofition of the elaftic fluid? This is a point which, in my opinion, the prefent ftate of our knowledge does not permit us to decide.

If it were permitted me to indulge in conjectures, I fhould fay, that fome experiments, which are not fufficiently complete to fubmit to public infpection, induce me to believe, that every elaftic fluid refults from the combination of of ſome ſolid or fluid body with the inflammable principle, or perhaps even with the matter of pure fire, and that on this combination the ſtate of elaſticity depends. I ſhould add that the ſubſtance fixed in metallic calces, and which augments their weight, would not be, properly ſpeaking, on this hypotheſis, an elaſtic fluid, but the fixed part of an elaſtic fluid, which has been deprived of its inflammable principle. The principal action of charcoal, and all other ſubſtances of that nature employed in reductions, would then be, to reſtore the phlogiſton, or matter of fire, to the fixed elaſtic fluid, and with it the elaſticity which depends on it.

However different this opinion may ſeem to be from that of M. Stalh, it yet perhaps is not incompatible with it. It is poſſible that the addition of charcoal in the reduction of metals may anſwer two purpoſes at once; 1ſt, That of reſtoring to the metal the inflammable principle which it has loſt: 2dly, That of reſtoring to the fixable elaſtic fluid in the metallic calx, the principle which conſtitutes its elaſticity. But I repeat it again that it is with great caution, that an opinion on ſo delicate, ſo difficult a ſubject ſhould be hazarded; a ſubject which is very nearly connected with one ſtill more obſcure, I mean the nature of the elements themſelves, or at leaſt of what we regard as elements. Time and experiment alone can ſettle our opinions on theſe points.

CHAPTER VI.

OF THE COMBINATION OF ELASTIC FLUID WITH METALLIC SUBSTANCES BY CALCINATION.

I HAVE hitherto proved the exiſtence of an elaſtic fixable fluid in the metallic calces, only by the ſeparation which takes place in the moment of reduction. Though the experiments I have related might appear, in this reſpect, of ſuch a nature as not to leave any doubt; it muſt neverthe-

leſs

lefs be confeffed; that conviction in phyfics is only produced in as much as we arrive at the fame point by different roads.

I THEREFORE intend to fhew in the courfe of this chapter, that as whenever a metallic calx paffes from that ftate to that of a metal there is a *difcharge* of elaftic fluid, fo alfo whenever a metal paffes from its metallic ftate to that of a calx there is an *abforption* of the fame fluid, and that the calcination is nearly proportionable to this abforption.

EXPERIMENT I.

THE CALCINATION OF LEAD, BY A BURNING GLASS, UNDER A GLASS RECEIVER INVERTED IN WATER.

THREE drachms of fheet lead were placed in the apparatus reprefented fig. 8th. and expofed to the focus of one of Tchirnaufen's large burning glaffes of 33 inches diameter, the fame as was mentioned above. The focus of this lens was contracted and made ſhorter by means of a ſecond which had been added at a ſuitable diſtance. A piece of hard ſtone, ſuch as is uſed to pave the ſtreets of Paris, ſerved to ſupport the lead; it was hollowed in the middle to prevent the metal from ſlipping off before it was fuſed.

The lead flowed at the inſtant it was preſented to the focus; ſoon after, a whitiſh vapour began to riſe which collected on the ſides of the receiver, and formed a yellowiſh ſediment. At the ſame time there was formed on the ſurface of the lead a thin ſtratum of calx, which as the calcination proceeded, became of a yellow maſſicot colour. Theſe different effects were produced during the firſt five minutes; after which, having continued to keep the lead exactly in the focus, I beheld with ſurpriſe, that the calcination did not proceed. I perſiſted in the purſuit of this experiment for half an hour, without perceiving that the ſtratum of calx which had formed on the lead, was in the leaſt increaſed. It may be imagined, that the air contained in the receiver

receiver muſt be much heated, and that from its rarefaction it muſt have lowered the ſurface of the water G. H. but in proportion as the veſſels cooled, it re-aſcended, and when the whole apparatus was returned to the ſame degree of temperature as before the operation, a diminution of about ſeven cubic inches was perceived in the volume of the air.

The lead, having been withdrawn, was found as malleable as before the operation, except the little coat of calx adhering to it, which was exceedingly ſmall. It had loſt nearly half a grain of its weight, but it was evident from inſpecting the yellow flowers which lined the dome of the receiver, that this diminution proceeded from the evaporation, and that, by adding their weight to that of the lead, there had been an augmentation of ſeveral grains.

EXPERIMENT II.

The Calcination of Tin.

TWO drachms of tin were expofed to the focus of the fame lens and under the fame apparatus. The calcination was ftill more difficult than that of lead; the metal was covered with a fmall but exceedingly thin coat of calx; and it fumed a little. The operation was continued for twenty minutes, and yet I did not perceive that the calcination had made any progrefs. When the veffels had recovered the temperature at which they were before the experiment, only a very fmall diminution was found in the volume of air. The tin having been weighed again, was, augmented about the eighth of a grain; it alfo continued as malleable as before the operation, and had but an extremely thin covering of calx on its furface.

EXPERIMENT III.

The Calcination of a mixture of Lead and Tin.

I WAS inclined to try whether the calcination of a mixture of tin and lead would not be more eaſily performed; and therefore compoſed an alloy of equal parts of lead and tin, and expoſed two drachms of it to the focus of the burning glaſs; the receiver was, at moſt, but half as large as that of the firſt experiment of this chapter, and was only $5\frac{1}{2}$ inches in diameter.

The mixture fuſed immediately; many white fumes aroſe, part of which adhered to the top of the receiver, and part were depoſited on the ſurface of the oil. The operation was continued for 20 minutes; after which the calcination appeared much more advanced than in the former experiments, and there were appearances like vegetation on the ſurface. The veſſels being cooled, a diminution was found in the bulk of of the air, from 5 to 6 cubic inches. The receiver contained a great quantity of flowers, and the button of tin and lead was diminiſhed 4 grains, which loſs ſeemed to be recovered with increaſe in the portion which was ſublimed. Though the calcination was rather farther advanced in this than in the preceding experiments, yet the greater part of the mixture was ſtill malleable, and in its metallic ſtate.

The preceding experiments, though they confirm thoſe of Chapter V. left me, notwithſtanding, yet diſſatisfied; 1ſt, Becauſe the ſurface of the oil, confined in the receiver, being expoſed to ſo conſiderable a degree of heat, might poſſibly produce air during the calcination, or elſe might abſorb it: 2dly, Becauſe the heat of the focus being too violent, it volatilized the lead and the tin, in proportion as they were calcined, in ſo much that I could not obtain any certain reſult of the increaſe of weight in theſe metals. I endeavoured to remedy both theſe inconveniences in the following experiment.

EXPE-

EXPERIMENT IV.

The Calcination of Lead under a Glass Vessel inverted in Quick-silver.

I MADE ufe of an apparatus nearly fimilar to that reprefented by fig. 8th. it differed however from it; 1ft, in having, inftead of the bucket or ciftern B. D. C. E. a ftrong earthen glazed pan: 2dly. In that, inftead of filling it with water, I poured into it 80 pounds of quick-filver: and, 3dly, That inftead of the receiver F. G. H. I fubftituted a glafs cucurbit with a flat bottom. The object of this laft alteration was to have a veffel of the fame capacity as the receiver, but whofe aperture fhould be narrower, that lefs mercury might be ufed. Thefe difpofitions being made, I placed on the column I. K. a ftone crucible containing 3 drachms of lead: the crucible was above an inch in diameter, and about four lines in depth: it was flat at the bottom, that the metal might prefent a larger furface to the fun's rays. I afterwards covered the whole with the glafs

glafs cucurbit which ferved me inftead of a receiver; the mercury was raifed by the fyphon, L. M. as high as G. H. and the point parallel to its furface was carefully marked with a flip of paper which went almoft round the veffel; and laftly, I prefented the whole apparatus to the great burning lens, obferving that the lead was a full inch from the true focus, and that the heat was not much greater than that neceffary to melt it.

At the very inftant that the lead melted, though it was taken out of the centre of a large piece, though it was bright on every fide, and had not the leaft appearance of foulnefs, a pellicle immediately formed on its furface. In the progrefs of the calcination this pellicle became of a yellow mafficot colour, and wrinkled on the fide towards the meridian; after which, at the end of ten or twelve minutes, the calcination ftopped, and no farther effect was obferved; only it happened that at thofe inftants when the heat was a little ftronger, the yellow pellicle fufed in fome places, and formed a yellowifh glafs; from the portions, thus vitrefied, fumes arofe

arofe plentifully, which tarnifhed the top of the cucurbit. I oppofed, as much as poffible, the evaporation, by removing the lead farther and farther from the true focus of the lens.

The lead was thus expofed to the action of the large burning glafs for one hour, forty-five minutes; but as, during this period, the fun was, at times, obfcured by fmall clouds, we cannot reckon on more than an hour and fifteen minutes of real effect.

The operation being finifhed and the veffels perfectly cooled, the furface of the mercury was found to have afcended two lines and half above its former level. The diameter of the cucurbit was, in this place, $4\frac{8}{10}$ inches, which makes the quantity of air abforbed to be $3\frac{3}{4}$ cubic inches. The lead having been carefully fepa-rated from the crucible, weighed 3 drachms $1\frac{3}{4}$ grain: I eftimated the yellowifh vapours ad-hering to the fides of the cucurbit at about $\frac{3}{4}$ of a grain; the total increafe of weight in the cal-cination had been, then, $2\frac{1}{2}$ grains, viz. $\frac{2}{3}$ of a grain for each inch of air. It refults, therefore, that

that the quantity abſorbed is exactly proportionate to the augmentation in weight of the metallic calx.

The void ſpace of the cucurbit, or, in other words, the volume of air in which the calcination was made, was 75 cubic inches; ſo that the abſorption was preciſely a twentieth.

EXPERIMENT V.

The Effect of Air in which Lead has been calcined, on Burning Bodies.

I CALCINED, as in the laſt experiment, and in the ſame apparatus, three drachms of lead. The operation being finiſhed, turning the cucurbit FGH. fig. 8th. briſkly, I placed it with its mouth uppermoſt, and immediately introduced a wax candle. It burnt tolerably well juſt at firſt, but it began inſenſibly to languiſh, and it was extinguiſhed in about a minute.

EXPE-

EXPERIMENT VI.

THE EFFECT OF AIR, IN WHICH METALS HAVE BEEN CALCINED, ON LIME-WATER.

THE procefs was conducted in the fame manner in this as in the preceding experiment, with this difference only, that inftead of introducing a candle into the cucurbit, I poured fome lime-water into it. Its mouth was then ftopped, and I fhook it ftrongly. The lime-water acquired a flight, but fcarcely perceptible turbid appearance, and no precipitation enfued.

IT is evident from thefe two experiments, that air in which metals have been calcined, is not, by any means, in the fame ftate as that feparated by effervefcence, and by metallic reductions.

EXPERIMENT VII.

The Calcination of Iron by Means of Moisture.

FOUR ounces of iron filings were put into a glafs veffel, and moiftened with a little diftilled water, and the whole covered with a glafs receiver, the vacant fpace of which was about 200 cubic inches. No fenfible effect was produced for fome days; the fineft particles of the iron fwam upon the furface of the water without being reduced to ruft, the remainder was at the bottom. At the end of eight days, a fmall quantity of ruft was formed, and the volume of air was diminifhed fix or eight inches, in fifteen days fifteen inches, in a month thirty-fix inches, and laftly, in two months the diminution amounted to about fifty inches; at this period the abforption ceafed to proceed, for at the end of feven months, the apparatus continued in the fame ftate, and the abforption had not, in the leaft degree, increafed.

It appears from theſe experiments: 1ſt, That the calcination of metals is not near ſo eaſily performed, when they are confined in a portion of air contained in a glaſs receiver, as in the open air.

2dly. That this calcination is alſo limited; viz. that when a certain portion of metal has been reduced to a calx in a given quantity of air, it is impoſſible to carry on the calcination farther in the ſame air:

3dly. That in proportion as the calcination proceeds, there is a diminution in the volume of air, and this diminution is nearly anſwerable to the augmentation of weight in the metal:

4thly. That by comparing theſe facts with thoſe contained in the preceding chapter, it ſeems proved, that an elaſtic fluid is combined with, and becomes fixed in metals during their calcination, and that to this fixation their augmentation in weight is to be attributed:

5thly. That ſeveral circumſtances would ſeem to

 lead

lead to a belief, that the whole of the air which we breathe is not adapted to be fixed, and enter into combination with metallic calces; but that there exifts in the atmofphere, an elaftic fluid of a particular kind which is mixed with the air, and that it is at the inftant when the quantity of this fluid contained under the receiver is confumed, that the calcination can no longer take place. The experiments which I fhall relate in Chapter IX. will give, at leaft, fome degree of probability to this opinion.

The experiments of which I have given an account, would appear alfo to lead to the two following confequences: 1ft, That the calcination of metals cannot take place in veffels clofely ftopped, or, at leaft, that it can only be in proportion to the quantity of fixable air which is confined in them: 2dly, That in cafe the calcination could proceed in veffels clofely ftopped and exhaufted of air, it fhould then be without increafe of weight, and confequently with circumftances very different from thofe obferved in calcination performed in air.

The train of experiments which Meſſieurs Darcet and Rouelle have announced in a memoir inſerted in the Journal de Medicine for the month of January laſt, on the calcination of metals in porcelain veſſels exactly ſtopped, will, without doubt, throw great light on this ſubject. Perhaps this calcination may be no more than a ſimple privation of Phlogiſton in the ſenſe which Stalh meant. However this may be; the learned cannot but expect with much impatience the publication of theſe experiments, and the reputation which theſe two chemiſts have ſo juſtly acquired, ſufficiently anſwer for the accuracy to be expected in them. *

CHAP-

* I had not the leaſt knowledge of Dr. Prieſtley's experiments, when I was employed in thoſe related in this chapter. He has obſerved with me and before me, as has been ſeen in the firſt part of this work, that a diminution in the volume of air was produced during the calcination of metals: this diminution in ſome experiments, was equal to $\frac{1}{5}$, and even to $\frac{1}{4}$ of the bulk of air which he had employed. Although I have made uſe of a lens, the ſtrongeſt of any known, I have not been able to carry it to above $\frac{1}{6}$

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CHAPTER VII.

EXPERIMENTS ON ELASTIC FLUID DISENGAGED FROM EFFERVESCENT MIXTURES, AND FROM METALLIC REDUCTIONS.

HAVING ſhewn that elaſtic fluid is ſeparated very plentifully in the reduction of minium, it remains that I ſhould give ſome experiments on the nature of that fluid, and eſpecially that

in the dry way. This circumſtance induced me to ſuſpect that the elaſtic, fixable, fluid which is diffuſed in the air is, perhaps, more abundant in it at one time and in one place than another; that it is mixed, in greater proportion, with the atmoſpheric air in inhabited places, in our laboratories, &c. than in fields, gardens, and in places in general where the air is perpetually renewed. Further, Dr. Prieſtley is perſuaded that the diminution in the quantity of air which he obſerved, proceeded from a ſuperabundance of Phlogiſton which was ſupplied to it by the calcination of that I ſhould prove its perfect identity with that diſengaged by effervefcence: but previous to entering on the detail of experiments, which I ſhall adduce in proof of it, it may be neceſſary to proceed here to ſome preliminary deſcriptions.

An Apparatus proper for obtaining the Elastic Fluid from Effervescing Mixtures as pure as possible, without making use of Bladders.

THIS apparatus is repreſented fig. 13th. ACB is a bottle containing about two pints, tubulated

of the metal, and he does not ſeem to have ſuſpected[1] that the calcination itſelf was an abſorption, a fixation of elaſtic fluid.

[1] At the time Mr. Lavoiſier wrote theſe obſervations, he had only ſeen Dr, Prieſtley's memoir publiſhed in the Philoſophical Tranſactions; theſe two great philoſophers ſeem to have made the diſcovery of the cauſe of the increaſe in the weight of metallic calces, nearly about the ſame time. They have both, ſince their reſpective publications, been employed in the farther purſuit of theſe reſearches, and I hope to be enabled, to add, by way of appendix to this tranſlation, an account of their diſcoveries. Mr. Lavoiſier has already publiſhed a memoir on this ſubject, in a periodical work, of which M. l' Abbé Rozier is the editor, entitled Obſervations ſur la Phyſique, ſur l' Hiſtoire Naturelle et ſur les Arts: and I have the pleaſure to find that a ſecond volume of Dr. Prieſtley's Experiments and Obſervations on Air, containing the moſt important diſcoveries on that ſubject, is in the preſs, and in great forwardneſs. T. H.

tubulated at E, the ſame of which a deſcription has been given above, fig. 4th. As much chalk, in groſs powder, is to be put into it as will fill it up to about a third, or at moſt one half of its capacity, and then the funnel G is to be luted to it in the ſame manner as in fig. 5th. and 7th.

Again, the bottle O is to be filled with pure water, inverted into an earthen bucket, VVFF, alſo full of water, and placed on a wooden ſtand or trivet, perforated in the middle, and weighted with lead to prevent it from floating. The communication between the bottles A and O is to be procured by means of the two bent tubes, EI and TXLM.

SS is a tube which is fitted by friction with great exactneſs to two other tubes IE and TX. The tube SS has a cock at R, which may be opened or ſhut at pleaſure.

When all the junctures are exactly ſecured with the fat lute covered with moiſtened bladders, as much dilute vitriolic acid is to be introduced into the bottle A, by means of the funnel

G,

G, as is ſufficient to produce a quantity of elaſtic fluid, at leaſt, capable of filling the vacant ſpace in the veſſels, and of driving off the common air which they contain. This being done, the mouth of the funnel is to be ſtopped, with the cork P, fig. 5th. and it muſt be filled with dilute vitriolic acid; after which, by means of the ſmall rod, OP, which is faſtened to the cork P, the neceſſary quantity of vitriolic acid is permitted to enter into the bottle A. At the ſame time we muſt not forget to open the cock R.

As faſt as the elaſtic fluid is ſeparated from the chalk in the bottle A, it paſſes into the bottle O and drives out the water in proportion. It is neceſſary in ſome experiments to introduce, into the bottle O, a thin covering of oil, which ſwimming on the ſurface of the water, may prevent the elaſtic fluid from coming into actual contact with it.

THE

The Manner of preserving the Elastic Fluid in Bottles, for any Length of Time we please.

WHEN all the water has been expelled from the bottle O, fig. 13th. by the elaſtic fluid, and there only remains a thin coat of oil in the neck, the extremity M of the ſyphon TXLM is to be withdrawn, the bottle corked under water, and afterwards removed wherever is thought proper. The elaſtic fluid may be preſerved a very long time in this ſtate. However, when it is to be kept from one ſeaſon to another, and obliged to undergo the changes of heat and cold, it is neceſſary to take ſome farther precautions; as this air is ſuſceptible of condenſation by cold like that of the atmoſphere, the external air, when the weather is very cold, preſſes on the cork, and it is difficult to prevent it, in time, from entering into the bottle and mixing with the elaſtic fluid contained in it. It is eaſy to avoid this mixture of the two airs, by plunging the bottles of elaſtic fluid with their necks downwards either into a diſh or jar of water, as may be be ſeen repreſented fig. 14th. In experiments where we are not apprehenſive of the ſmall loſs of elaſtic fluid cauſed by the abſorption of the water, we diſpenſe with the omiſſion of the bed of oil in the neck of the bottle; this precaution may even become injurious if we intend to keep the elaſtic fluid for a very conſiderable time; for the oil being liable to ferment and be corrupted, it might produce particular phenomena. It is, then, neceſſary to leave a very ſmall quantity of water in the neck of the bottle, inſtead of the coat of oil.

The Method of removing the Elastic Fluid from one Vessel to another.

LET the recipient *n*NOO, fig. 10th. be ſuppoſed to contain a certain quantity of elaſtic fluid, which there is occaſion to transfer into a jar, bottle, or any other veſſel: a communication is to be formed by means of the bent tube EBCD and the tube *SS* furniſhed with its cock, between the inſide of the receiver *n*NOO and the body of the pump P, as alſo, by means of the

the tube *SS* and that marked *txlm*, between the pump P and the veſſel Q, which ſhould be exactly filled with water; laſtly, the piſton Z of the pump P is to be ſet in action, and every time the piſton is raiſed, the air of the recipient *n*NOO paſſes into the body of the pump P; it is then driven on, and obliged to paſs into the veſſel Q from which it diſplaces the water in proportion. If the veſſel made uſe of be a bottle, it may be corked under the water, and the elaſtic fluid preſerved, in the manner which has been before deſcribed.

A Description of an Apparatus for transmitting an Elastic Fluid through any Kind of Liquor, and afterwards preserving it for Examination.

THIS apparatus repreſented fig. 15th. differs in nothing from that of the laſt experiment except in the bottles *p'*, *p''*, *p'''*, which are placed between the pump PP and the bucket *nnff*. Theſe bottles are entirely ſimilar to that of fig. 4. They are to be filled with lime-water, or any

any other liquor through which the operator chuſes to tranſmit the elaſtic fluid. A communication is to be made from the pump PP to the firſt bottle, by means of the bent tube *mp* repreſented ſeparately fig. 16th. Laſtly, when by the action of the piſton Z, the elaſtic fluid has paſſed into the body of the pump P, and it is again filled, it is neceſſarily obliged to paſs into the tube *m′p′*, and to bubble over into the liquor contained in the bottle *p′*; the preſſure of the ſucceeding ſtreams of air forces it to continue its paſſage, and to riſe over ſucceſſively into each of the bottles *p″*, *p‴*, and ſo on into as many as may be thought proper, till, in fine, all the air, which could not be abſorbed by the liquor, paſſes into the bottle or jar Q, through the tube *txlm* fig. 17th.

The different inſtruments which I have deſcribed, changed and modified in various manners, have been ſufficient for almoſt all the experiments which I have been obliged to make on the elaſtic fluid diſcharged from bodies. Indeed thoſe relative to Dr. Prieſtley's nitrous and inflammable airs ſhould be here excepted, which have not as yet been the objects of my examination,

ation, and which require particular precautions. I thought it neceſſary to begin with theſe deſcriptions, that there might be no occaſion to return to them in the courſe of this Chapter, nor any neceſſity to interrupt the recital of my experiments.

EXPERIMENT I.

THE EFFECT OF ELASTIC FLUID DISCHARGED FROM CHALK ON ANIMALS.

A QUANTITY of elaſtic fluid was ſeparated from chalk by the vitriolic acid, and tranſmitted by means of the apparatus fig. 13th. into the jar Q fig. 15th. repreſented ſeparately fig. 18th. The jar was corked under water with a large cork exactly fitted to it; after which I turned it up, pulled out the cork and immediately introduced a young ſparrow.

IT had ſcarcely reached the bottom of the jar before it fell on its ſide in convulſions; and having withdrawn it in a quarter of a minute, it

was

was expiring, and I could not poffibly reftore it to life by any method.

The fame experiment being repeated on a rat, it perifhed with the fame circumftances, and nearly in the fame fpace of time. Its fides were fhrunk, and had a kind of convulfive motion as if it had endeavoured to infpire the air without being able to accomplifh it.

EXPERIMENT II.

The Effect of Elastic Fluid, disengaged from Metallic Calces, on Animals.

THE fame jar Q was filled with elaftic fluid difengaged in the reduction of minium, and a fparrow, a moufe and a rat were fucceffively introduced into it. They died almoft inftantly, as in elaftic fluid difcharged by effervefcence, and their death was attended by fimilar circumftances.

These experiments feem to difcover one of the principal caufes of the almoft immediate death

death of animals in the elaſtic fluid of effervefcing mixtures and metallic reductions. Without knowing precifely what is the ufe of refpiration to animals, we at leaſt know that this function is fo effential to their exiſtence, that they muſt very foon periſh if their lungs be not inflated almoſt every moment by the elaſtic fluid which compofes our atmoſphere; but it may eafily be conceived that the elaſtic fluid from effervefcence, or that from metallic reductions, is not by any means proper to perform that office of the animal œconomy, and cannot inflate the lungs of animals like the air which we commonly breathe. We have actually feen above, that this fluid is abforbed with great facility by water and moſt other liquors, that it fixes itfelf with them and fuddenly lofes its elaſticity: hence it neceffarily refults that the interior part of the lungs being compofed of moiſt membranes, and even of veffels through which watery vapours continually tranfude; the elaſtic fluid cannot arrive there without fuddenly lofing its elaſticity: indeed it is even probable that the elaſtic fixable fluid does not reach the laſt ramifications of the lungs, but is fixed before it come there. The action of the lungs

lungs then muſt be ſuſpended by the deficiency of elaſtic fluid, they muſt collapſe and become flaccid; and this is, in fact, what is obſerved in the diſſection of animals which have periſhed in this way. Almoſt the ſame effect would be experienced in a pair of bellows, the inſide of which was moiſtened with water, and its action attempted to be ſupported with elaſtic fixable fluid.

EXPERIMENT III.

The Effect of Elastic Fluid separated from Effervescing Mixtures on Burning Bodies and on Flame.

A LONG, narrow jar, fig. 19th. was filled with elaſtic fluid diſcharged from chalk, and I plunged into it a lighted wax taper or candle, fig. 20th. ſuſpended by means of an iron wire.

Scarcely was it arrived at the mouth of the jar, but it was inſtantly extinguiſhed; the burnt part of the wick was even become black. It

 ſometimes

ſometimes happened that I could light the ſame candle ten or twelve times, and extinguiſh it as often in the ſame jar; ſo true it is that a conſiderable time is neceſſary for the elaſtic fixable fluid to be mixed with the air of the atmoſphere: only it is to be obſerved that every time the candle is extinguiſhed anew, it is neceſſary to immerſe it lower than the preceding time, which ſeems to prove that the union of elaſtic fluid with atmoſpheric air is only made on the ſurface, and one ſtratum after another, nearly in the manner in which ſolution is performed.

A RED hot piece of charcoal immerſed in the ſame air, became black in it immediately, as if it had been plunged into water.

EXPERIMENT IV.

THE EFFECT OF ELASTIC FLUID DISCHARGED FROM METALLIC CALCES ON BURNING BODIES AND ON FLAME.

THE ſame experiment was repeated, making uſe of elaſtic fluid diſcharged from minium inſtead

ſtead of that from chalk; the effects were preciſely the ſame, nor did I perceive the leaſt difference.

EXPERIMENT V.

To transmit Elastic Fluid, disengaged from an Effervescing Mixture, through Lime-Water, and to observe the Quantity absorbed by it.

I FILLED a bottle, whoſe capacity was 206 $\frac{1}{2}$ cubic inches, with elaſtic fluid ſeparated from chalk by vitriolic acid; it was placed with its mouth downwards in a bucket full of water, VV, fig. 15th. and the whole was diſpoſed in the manner deſcribed at the beginning of this Chapter. The jar Q was 69 cubic inches in capacity, it was exactly filled with water, and the three bottles p', p'', p''', contained 7 $\frac{1}{2}$ pints of lime-water. When every thing was thus prepared, and all the junctures were exactly cloſed with fat lute, I opened the cocks R r and worked the piſton Z of the pump P.

As ſoon as the air came over into the three bottles p', p'', p''', and at the firſt ſtroke, the firſt bottle began to aſſume a cloudy appearance; the ſame happened to the ſecond towards the end of the ſecond ſtroke, and to the third on the fourth. I was obliged to make fifteen ſtrokes and a half of the piſton to fill the jar Q with elaſtic fluid.

The capacity of the pump is $12\frac{1}{7}$ inches, from whence it follows that the quantity of elaſtic fluid which I had cauſed to paſs into the lime-water was 188 inches, the air which had ſeparated from the lime-water was 69 inches, the quantity then which was combined with the lime was 119 or about two-thirds.

It is proper to obſerve that this experiment does not give very exactly the portion of elaſtic fluid, capable of being abſorbed by the lime; in fact, a portion of air contained in the vacant part of the bottles p', p'', p''', paſſes into the jar Q, and is replaced by elaſtic fluid; and conſequently the quantity of elaſtic fluid abſorbed appears leſs than it really is. It is, moreover, probable

probable that $7\frac{1}{2}$ pints of lime-water would not be ſufficient to deprive the elaſtic fluid of all the portion capable of being fixed, and that ſome ſmall part ſtill penetrates into the jar Q. It is doubtleſs for theſe various reaſons that the elaſtic fluid was only reduced $\frac{2}{3}$ in this experiment, whereas Dr. Prieſtley was able to reduce it $\frac{4}{5}$.

EXPERIMENT VI.

The Effect of Elastic Fluid from Effervescing Mixtures on Animals, after it has been deprived of its Fixable Part by Lime.

WHEN the water in the jar Q, fig. 15th. was wholly diſplaced by the elaſtic fluid which had paſſed through lime-water, I was curious to try what effects it would produce on animals, and I accordingly withdrew the jar from the water, after having corked it, as has been before deſcribed, and I introduced a young ſparrow into it; it did not appear to ſuffer very ſenſibly at firſt, but in about half a minute its reſpiration ſeemed difficult, it opened its bill, and in about a minute it fell on its ſide almoſt motionleſs. It was left in this ſtate fully half a minute longer, after which it was taken out and expoſed to a current of freſh air. For the firſt moments it had no motion except of its eyes, and, in a ſmall degree alſo, of its bill, but in leſs than a minute it came to itſelf and began to hop and fly.

EXPERIMENT VII.

THE EFFECT OF THE SAME FLUID ON FLAME.

A SMALL portion of elaſtic fluid from chalk which remained in the bottle *A*, fig. 15th. was paſſed through the ſame lime-water, and afterwards collected in a ſmall jar: a ſmall taper, which was immerſed in it in the manner repreſented fig. 19th. and 20th. was extinguiſhed in an inſtant.

THE lime-water, which had ſerved for theſe experi-

experiments, and was contained in the bottles *p′*, *p″*, *p‴*, was found to be entirely deprived of its alkaline tafte. The lime which was precipitated made a brifk and long continued effervefcence with acids, and after all the trials to which it was expofed, I could not find that it differed in any refpect from chalk.

EXPERIMENT VIII.

TO MAKE ELASTIC FLUID, SEPARATED FROM A METALLIC CALX BY REDUCTION, PASS THROUGH LIME-WATER; TO OBSERVE THE QUANTITY ABSORBED BY IT, AND THE EFFECT OF THE RESIDUUM ON ANIMALS AND ON FLAME.

INSTEAD of the bottle A, fig. 15th. I made ufe of a large receiver *n*NOO, fig. 10th. into which I transferred a mixture of 560 cubic inches of elaftic fluid feparated from a metallic calx, and 80 cubic inches of common air. It would doubtlefs have been preferable to have ufed elaftic fluid pure and unmixed, but the apparatus defcribed above, fig. 10th. did not permit me to obtain it fo, becaufe fome common air always neceffarily remained in the empty fpace of the retort A, and in the tubulated receiver GH. The large fyphon EBCD was adapted in the fame manner as in the former experiment to the pump PP, fig. 10th. and 11th. and the elaftic fluid was made to pafs through four bottles, each of which contained two pints ten ounces of lime-water; the jar Q, the capacity of which was 66 inches, was then difpofed to receive the air which could not be abforbed by the lime-water.

At the firft ftroke of the pifton, the lime-water contained in the firft bottle, began to lofe its tranfparency, and grew fenfibly turbid on the fecond.

The water in the fecond bottle began to be cloudy at the third ftroke of the pifton, that in the third on the fourth ftroke, and that of the fourth on the fixth.

It was requifite to pump 135 cubic inches of elaftic elaſtic fluid, to diſplace all the water contained in the receiver Q and to fill it with air; hence it appears that 135 cubic inches had been reduced to 66 inches in paſſing through the lime-water, and therefore that 69 inches of air had been combined and fixed either with the lime or with the water.

A RAT having been put into this air, continued ſufficiently eaſy at firſt. It then appeared to ſuffer, and was violently agitated; at length, in three or four minutes, it fell down in a kind of ſtupor, and continued without motion as if dead. Having withdrawn it, it began, in a few minutes, to ſhew ſome ſigns of life; it afterwards recovered gradually, and preſently became as lively as before the operation.

A WAX candle lighted and plunged into the ſame air was immediately extinguiſhed.

THE water in the two firſt bottles *p'*, *p"*, at the end of this operation, had already formed a very conſiderable ſediment; that in the third and fourth was already very turbid; but one might eaſily eafily judge that all the lime, which was diffolved there, was not yet precipitated. I therefore endeavoured to tranfmit again more elaftic fluid through the fame water into the jar Q; the quantity of air neceffary to fill it was found to be 120 inches, and confequently, this fecond time, no more than 54 inches were abforbed, or precifely $\frac{45}{100}$.

I FILLED the fame jar Q a third time in like manner, and the quantity of elaftic fluid abforbed by the lime, in this operation, was but 48 inches, viz. $\frac{42}{100}$.

THE fame rat, having been put into this air, feemed to feel much greater inconvenience from it; in lefs than a minute it fell on its fide; I withdrew it, but it was dead, and it was no longer poffible to reftore it to life.

THE fame jar was again filled a fourth time in the fame manner; 44 inches only were now abforbed, viz. exactly $\frac{4}{10}$ of the quantity employed. A moufe being placed in this air, perifhed in the third part of a minute.

THE

The quantity of elaſtic fluid, neceſſary to fill the jar Q the firſt time, was 135 cubic inches; but it muſt be remembered that this elaſtic fluid contained $\frac{1}{7}$ of common air; the 135 cubic inches were therefore compoſed of $115\frac{5}{7}$ inches of elaſtic fluid ſeparated from the calx of lead, and of $19\frac{2}{7}$ inches of common air: but, on the other ſide, common atmoſpherical air is not capable of uniting ſuddenly with lime-water, like the elaſtic fluid ariſing from effervеſcence and reductions; therefore the $19\frac{2}{7}$ inches of common air, after having bubbled through the lime-water, ſhould paſs, without diminution, into the jar Q. It is evident from this calculation, that in reality 135 inches of elaſtic fluid were not reduced to 66 inches, but $115\frac{5}{7}$ were reduced to $46\frac{5}{7}$. The lime-water, therefore, abſorbed $\frac{6}{10}$ of the quantity of elaſtic fluid which was employed.

When this calculation is applied to the ſecond, third and fourth times of filling the jar, it will be found that the quantity of elaſtic fluid uſed for the ſecond was 103 inches; that it was reduced to 49; from whence it follows that the quantity

quantity abſorbed by the lime-water was 54 cubic inches or $\frac{52}{100}$. That for the third the quantity of elaſtic fluid employed was 98 inches, which was reduced to 50, ſo that the quantity abſorbed was 48 inches or ſomething leſs than a half; and laſtly, that for the fourth, the quantity of elaſtic fluid expended was 94, and was reduced to 50 inches, viz. the quantity abſorbed by the lime was 44 inches or the $\frac{47}{100}$.

One remarkable circumſtance, which I have taken notice of above, is that the water in the bottles p', p'', p''', which became very turbid at the beginning of theſe different operations, and had depoſited all the lime which it contained in a ſtate of ſolution, towards the end of them grew gradually tranſparent. The reaſon of this phenomenon depends on the elaſtic fluid with which the water was impregnated, and by the aſſiſtance of which it became capable of diſſolving the calcareous earth. Some obſervations on this ſolution will be found in the next Chapter.

EXPE-

EXPERIMENT IX.

The Effect of a very great Degree of Cold on Elastic Fluid from Effervescence.

FIG. 21ſt. repreſents an apparatus which I judged neceſſary for this experiment. A is a bottle filled with elaſtic fluid ſeparated from chalk by vitriolic acid; the tube EBCD is exactly luted to it with the fat lute covered with a bladder, and it is adapted at its extremity D to the tube *SS* furniſhed with its cock R. Every thing being thus prepared, the bottle A was placed in a bucket which I filled with a mixture of broken ice and ſea ſalt.

Reflecting afterwards on this experiment, I conſidered that its principal end was to contract the elaſtic fluid and condenſe it as much as poſſible; that, however, as the air in the bottle A had no communication with the external air, my object would not be accompliſhed. Indeed, whatever

whatever degree of cold I had applied to it, in this apparatus, its volume would always have remained equal to the capacity of the bottle. From theſe conſiderations I perceived that it was indiſpenſably neceſſary, in order to procure any advantage from this experiment, to lute to the other extremity of the tube *SS*, a ſyphon TXLM which might communicate with the inſide of the inverted bottle O filled with elaſtic fluid alſo diſcharged from chalk. I then opened the cock R. It is evident that by means of the communication formed between the bottles A and O, the elaſtic fluid could not be condenſed by cold in the firſt; but a portion of that contained in the ſecond muſt paſs to fill up the vacuum; by which means the condenſation might be made with all poſſible freedom.

The air of the laboratory was at 10½ degrees above the freezing point. When I began this experiment, the degree of cold was at about 15 degrees below freezing. I continued to ſupport it at this temperature for five hours, without the elaſtic fluid being more diminiſhed than common air would have been. Having at this time re-

moved

moved the ice which furrounded the bottle, I found it covered internally with a white efflorefcence, which was nothing but the moifture of the air which was condenfed by the cold and had formed a kind of hoar froft.

It was afterwards defirable to try whether the cold had changed the nature of the elaftic fluid, and whether it had become more fimilar to that of the atmofphere, as the Count de Saluces had afferted. (See Part I. page 47.) For this purpofe I replaced the bottle A in an earthen bucket VV filled with water, fig. 15th. the elaftic fluid was then drawn off by means of the pump PP, and made to pafs through three bottles p', p'', p''', filled with lime-water.

At the firft ftroke of the pifton the liquor began to become cloudy, and afterwards became turbid in the fame manner as if the elaftic fluid had not been expofed to the action of cold. I alfo tried the effect of this fluid on animals; they perifhed in it in a few feconds, and it inftantly extinguifhed flame.

It

It appears from the experiments contained in this Chapter, 1ſt. That there is a perfect reſemblance between the elaſtic fluid diſengaged in the reduction of minium, and that ſeparated from effervefcing mixtures; and that they both produce the ſame phenomena on lime-water, on calcareous earth, on burning bodies and on animals.

2dly. That both theſe fluids are compoſed, 1ſt. Of a fixable part capable of being combined with water, &c. 2dly. Of another part, much more difficult to fix; capable of ſupporting, in a certain degree, the lives of animals, and in its nature much reſembling the air of the atmoſphere.

3dly. That this portion of common air is rather more conſiderable in the elaſtic fluid diſengaged in metallic reductions, than in that detached from chalk.

4thly. That it ſeems certain that the noxious property of this fluid reſides in its fixable part, becauſe it is leſs fatal to animals in proportion as it it is farther deprived of this part, as is proved by Experiment VIII.

5thly. THAT nothing as yet enables us to decide whether the fixable part of elaftic fluid from effervefcing mixtures and reductions, be a fubftance effentially different from air, or whether it be air itfelf to which fomething has been added, or from which fomething has been fubftracted, and that prudence demands us to fufpend our judgment, at prefent, on this fubject *.

* THIS point has been much elucidated by Dr. Prieftley's experiments.

CHAPTER VIII.

Of some PROPERTIES of WATER impregnated with ELASTIC FLUID separated from EFFERVESCING MIXTURES or METALLIC REDUCTIONS.

MESSIEURS Cavendiſh, Prieſtley, and Rouelle have communicated to the public ſome very intereſting experiments on the ſolvent properties of water impregnated with fixed air, or, in other words, elaſtic fluid, ſeparated from efferveſcing mixtures; they have demonſtrated that this water has the property of diſſolving calcareous earths, iron, zinc, iron ore, &c. I have had the curioſity to vary theſe experiments, to extend them, if poſſible, and I have endeavoured to form a three-fold union of fixed air, metals and acids, with a view of acquiring ſome ideas of the degree of affinity between theſe different ſubſtances.

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To accompliſh this deſign, I firſt impregnated a ſufficient quantity of pure diſtilled water with elaſtic fluid ſeparated from an effervеſcing mixture. For this purpoſe, I made uſe of the apparatus, fig. 7th.

Some of this water was poured into glaſſes, in which I had previouſly placed ſolutions of iron, copper, and zinc, in the vitriolic acid, ſolutions of iron, copper, lead, and quick-ſilver, in the nitrous acid, of gold in aqua regia, and of corroſive ſublimate: in whatever proportions theſe mixtures were tried, I was never able to produce a precipitation, and the liquors remained as tranſparent as they were before; nay, the ſolution of iron in the vitriolic acid, which was rather cloudy, became immediately clear by the mixture of water impregnated with elaſtic fluid.

I mixed ſome of the ſame water with a ſolution of ſilver in nitrous acid; the liquor had a ſlightly cloudy appearance, but ſcarcely perceptible, and the moſt ſcrupulous attention was requiſite to remark it. This circumſtance might cauſe a ſuſpicion, that chalk contains ſome particles of marine acid, which being combined in it with a bafis, is driven from it by the vitriolic acid, and paffes over with the elaftic fluid, and that uniting with the filver, in this experiment it forms a *luna cornea*; but fuppofing this fufpicion to be well founded, that quantity of marine acid muft be fo inconfiderable, that one grain of fpirit of falt diffufed in two pints of water would produce a much greater effect.

Though thefe experiments may not be quite complete, becaufe I have not been able to extend them to all the metallic folutions; they fhould feem, however, to prove in general, that metallic fubftances have more affinity with the mineral acids, than with the elaftic fixable fluid.

Mr. Hey, fome of whofe experiments have been publifhed by Dr. Prieftley, has declared that fixed air did not at all change the blue colour of fyrup of violets, and as this experiment has been fince contefted, I was curious to repeat it: fome fyrup of violets was accordingly diffufed in water impregnated with elaftic fluid, and its colour compared with that of the fame fyrup of violets diffufed in diftilled water. The colour was not fenfibly changed; however regarding it with

with the moſt nice attention, the ſyrup of violets mixed with water impregnated with fixed air, ſeemed to have ſomething of a redder tinge; but the difference was ſo trifling, ſo imperceptible, that one might almoſt doubt it*.

An experiment may be recollected which I have related in the firſt Chapter of the ſecond part of this work. If water impregnated with elaſtic fluid be poured by degrees into ſaturated lime-water, the liquor preſently becomes turbid, and the lime precipitates in the ſtate of chalk; but if, after all the chalk has been precipitated, the addition of more water impregnated with fixed air be continued, all the chalk which had been precipitated will be gradually rediſſolved, and the liquor recover its former tranſparency.

It has been alſo ſeen in the laſt Chapter, that when

* Dr. Priestley's very ingenious friend Mr. Bewley, of Great Maſſingham in Norfolk, ſeems to have determined this point. By means of fixed air, he has not only turned ſome of the blue juices, which are more delicate teſts of acidity than ſyrup of violets, red, but has even neutralized alkaline ſalt. T. H.

when elaſtic fluid, ſeparated either from effervefcence, or metallic reduction, has been tranſmitted through lime-water, and all the alkaline earth has been thereby precipitated under the form of chalk, if we continue to throw in freſh ſtreams of elaſtic fluid, the greater part of the precipitated earth is again diſſolved, and the liquor recovers its tranſparency. The elaſtic fluid or fixed air being ſo common in the mineral kingdom, as may be judged from the mephitic or aërial waters and by many other phenomena in nature, the combination of this ſubſtance with calcareous earths ſhould be frequently met with in waters; I therefore thought that it would be intereſting to examine the effects which different kinds of teſts would produce on this combination which is hitherto but little known.

To this purpoſe ſome diſtilled water was ſaturated with lime, and I threw a ſtream of air into it proceeding from a reduction of calx of lead; at firſt, as has been before remarked, the lime was precipitated; it was then rediſſolved, and I continued the operation in this manner till I imagined the water was as much loaded as it could be, with calcareous earth.

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I POURED this water on ſolutions of iron and copper in the nitrous acid, the liquors neither became turbid nor formed any precipitation. The ſolution of ſilver in the ſame acid occaſioned a ſlight, but ſcarcely perceptible cloud in the liquor, nearly ſuch as I have remarked with the water impregnated with elaſtic fluid only.

THE caſe was different with ſolutions of copper, iron, and zinc in the vitriolic acid. The precipitation, it is true, did not inſtantly take place, but in a few ſeconds the liquor became turbid, and in a ſhort time the precipitate collected and ſubſided to the bottom of the veſſel.

A SOLUTION of lead in the nitrous acid afforded, immediately, a very plentiful white precipitation.

A SOLUTION of quickſilver in nitrous acid afforded a precipitate only by employing much water and little of the ſolution; this precipitate was of a pale yellow colour, but it gradually became grey with time.

A solution of gold in aqua regia gave no ſigns of precipitation.

I also tried on this water the effects of fixed and volatile alkalis both in their cauſtic and mild ſtate; they all occaſioned a precipitation of the alkaline earth in the form of chalk; that is to ſay, they attracted to themſelves the ſuperabundant portion of elaſtic fluid which held it diſſolved, but they could not deprive it of any more; we have ſeen, in fact, that elaſtic fluid has more affinity with alkaline earth than with alkaline ſalts.

The ſame water being poured on ſyrup of violets, did not much affect its colour; however, a ſlight green tinge might be obſerved, which became more viſible after ſome hours.

All theſe experiments have the ſame ſucceſs, whether we employ elaſtic fluid ſeparated from effervefcing mixtures, or that from metallic reductions.

CHAPTER IX.

ON THE BURNING OF PHOSPHORUS, AND THE FORMATION OF ITS ACID.

EXPERIMENT I.

THE BURNING OF PHOSPHORUS UNDER A RECEIVER INVERTED IN WATER.

EIGHT grains of Kunkel's phoſphorus were placed on a little agate cup which was put under a glaſs receiver inverted in water, and a thin covering of oil was introduced to the ſurface of the water by means of a crooked funnel: this apparatus is the ſame as that repreſented fig. 8. I then threw upon the phoſphorus the focus of a glaſs lens of eight inches diameter.

THE phoſphorus was ſoon fuſed, and then kindled,

kindled, yielding a beautiful flame; at the ſame time a great quantity of white vapours aroſe, which ſettled on the internal ſurface of the receiver and tarniſhed it. Theſe vapours afterwards ran in *deliquium*, and formed drops of clear limpid liquor. At firſt, the water in the receiver rather fell, from the rarefaction occaſioned by the heat; but it preſently began to reaſcend ſenſibly, even during the burning, and when the veſſels were grown cold it ſettled at one inch five lines above its firſt level.

The internal diameter of this receiver was $4\frac{2}{10}$ inches; and conſequently the abſorption of air had been $19\frac{2}{3}$ inches. Having taken the cup from under the receiver, a yellow matter was found at the bottom which was nothing but the phoſphorus half decompoſed; I waſhed and dried it, after which it weighed between one and two grains, and therefore there had been only, in reality, between ſix and ſeven grains of phoſphorus burnt, and the abſorption of air had been about three inches for every grain of phoſphorus.

The part of the receiver above the water contained about 109 cubic inches. The absorption, then, of air was $\frac{2}{11}$, or, what is the same thing, between a fifth and sixth of the whole quantity of air contained under the receiver.

EXPERIMENT II.

The Burning of Phosphorus in a Receiver inverted in Quicksilver.

THIS experiment was repeated with the same receiver as in the last; I again employed eight grains of phosphorus; and all the circumstances were absolutely the same, with this difference only, that instead of inverting the receiver into a vessel of water with a covering of oil, it was inverted into a vessel full of quicksilver.

The burning succeeded nearly as in the last experiment, with this difference, that the vapours which adhered to the receiver were more light and flocculent, much whiter, and did not run

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per deliquium. Independent of thoſe attached to the receiver, the little cup was covered with them. The abſorption of air was 16¾ cubic inches, viz. rather leſs than three inches for every grain of phoſphorus. There alſo remained in the cup a ſmall yellow reſiduum of half decompoſed phoſphorus.

EXPERIMENT III.

THE BURNING OF PHOSPHORUS OVER QUICKSILVER, IN SMALLER QUANTITY THAN IN THE FORMER EXPERIMENTS.

I TRIED to burn a ſmaller quantity than eight grains of phoſphorus under the ſame receiver and over quickſilver. The quantity of air abſorbed was diminiſhed in proportion to the diminution of the quantity of phoſphorus, and it was conſtantly between 2¼ and 2¾ inches for each grain, deducting for the ſmall portion of yellow reſiduum which remained after each burning.

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EXPERIMENT IV.

To determine the greatest Quantity of Phosphorus which can be burnt in a given Quantity of Air, and what are the Limits of the Absorption.

TWENTY-FOUR grains of phoſphorus were placed on the agate cup in the ſame apparatus immerſed in mercury.

The phoſphorus burned in the ſame manner at firſt as if the quantity had been but ſix or eight grains, excepting only that the inflammation was more rapid, more inſtantaneous, and that the rarefaction was greater; but preſently, though a conſiderable quantity of phoſphorus ſtill remained unburnt, the combuſtion ceaſed, and I could not poſſibly renew it by the aid of the burning glaſs. I eaſily melted the phoſphorus, made it bubble, and even ſublimed it, but it no longer flamed. The quantity of air abſorbed in this experiment was found to be about ſeventeen feventeen or eighteen inches, and on comparing the remainder of the phofphorus with that which I had made ufe of, the quantity which was burnt was found to be no more than fix or feven grains.

These experiments were very frequently repeated, and the refults were always the fame, except fome difference in the quantities of air abforbed; it was never poffible for me to carry this abforption farther than twenty or twenty-one inches, or nearly approaching to, but not entirely, $\frac{1}{5}$ of the whole volume, in a receiver whofe capacity was 109 inches. Frequently, when the veffels had been fuffered to cool for feveral hours, I endeavoured to reftore the air under the receiver by lifting it up. As foon as the phofphorus came into contact with the frefh air, it immediately kindled again, and when I had covered it with another receiver of about the fame fize, fix or eight grains more were burnt. The phofphorus was then extinguifhed without the poffibility of being lighted again, by any other method than fupplying it with frefh air.

THESE experiments ſeem already to lead to a ſuſpicion that atmoſpheric air, or ſome other elaſtic fluid contained in the air, is combined, during the combuſtion, with the vapours of the phoſphorus. But there is a great difference between conjecture and proof, and it was eſſentially requiſite that it ſhould firſt be well eſtabliſhed that a combination of any kind of ſubſtance was formed with phoſphorus during its combuſtion. The following experiments appeared to me to be proper for furniſhing that proof.

EXPERIMENT V.

TO DETERMINE, WITH AS MUCH PRECISION AS THE NATURE OF THE EXPERIMENT WILL ADMIT OF, THE AUGMENTATION IN WEIGHT OF THE ACID VAPOURS OF BURNING PHOSPHORUS.

EIGHT grains of phoſphorus were put into a ſmall glaſs cup, B, fig. 22. which was introduced into a wide mouthed bottle, P. The bottle was very exactly ſtopped with a cork, and

and I weighed the whole to the exactneſs of half a grain. The bottle was then uncorked, and immediately placed under the cryſtal receiver ACG, which had been before made uſe of. The quickſilver was raiſed up to CG, and the phoſphorus kindled by the burning glaſs.

The phoſphoric acid was ſublimed in white flocculi, which were moſtly attached to the interior ſides of the bottle P, and to the cup B; one-fourth, at leaſt, was ſeparated on the outſide of the bottle, and was depoſited partly on the ſurface of the quickſilver, partly on the inner ſides of the receiver, and on the exterior ſurface of the bottle.

When the veſſels were grown cold, the abſorption was found to be from ſixteen to ſeventeen cubic inches, and a ſmall portion of yellow matter remained unburnt. I then lifted up the receiver A with proper precaution, and in leſs than four ſeconds, I recorked the bottle. It may eaſily be ſuppoſed that in ſo ſhort a time the air contained in the bottle P could not have been renewed and replaced by the moiſt air, or

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at leaſt that if ſuch an effect could have taken place, it muſt have been only in a quantity nearly inſenſible.

The outſide of the bottle P having been very exactly wiped and cleaned, I put it into the ſcales, and found it to have received an increaſe of weight of ſix grains; viz. that inſtead of eight grains of phoſphorus, which I had put into the bottle, there were now fourteen grains, either of concrete phoſphoric acid or of phoſphorus half decompoſed: but it muſt be remembered that at leaſt a fourth or three or four grains, was ſeparated during the burning, on the outſide of the bottle; and conſequently that ſix or ſeven grains of phoſphorus yield ſeventeen or eighteen grains of concrete phoſphoric acid, or in other words, that ſix or ſeven grains of phoſphorus abſorb ten or twelve grains of ſome ſubſtance contained in the air which is confined under the receiver. This experiment leaves ſcope enough to preclude any reaſonable doubt of the reſult, and all the arguments which could be adduced would, at moſt, tend only to reduce the increaſe of weight, from ten or twelve, to eight or ten grains.

The quantity of air abſorbed, was, at moſt, ſeventeen cubic inches combined with the phoſphorus to form the phoſpheric acid; this quantity communicated an increaſe in weight of from ten to twelve grains, and therefore every cubic inch of elaſtic fluid which was abſorbed weighed about $\frac{2}{3}$ of a grain, i. e. nearly one-fourth more than the air which we breathe.

But if the matter attracted by the phoſphorus, during its combuſtion, be the heavier part of the air, why may it not be water itſelf which that fluid holds diſſolved, and is diffuſed ſo abundantly in the atmoſphere in a kind of ſtate of expanſion? Without doubt, I reaſoned with myſelf, water is neceſſary to the aliment of flame; in proportion as air contains it, it is proper to ſupport combuſtion; but when deprived of it, combuſtion can no longer take place.

This ſentiment was probable, and carried an

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air of truth adapted to ſeduce. I therefore reſolved to ſubmit it to the teſt of experiment, and the following was my method of reaſoning. If this theory of the abſorption of water be true, three things ſhould reſult from it, 1ſt. That by reſtoring to the air confined under the receiver in which the phoſphorus has been burnt, a quantity of water reduced to vapours proportionable to that which has been abſorbed, the combuſtion, inſtead of ceaſing, ſhould be prolonged much farther. 2dly. That in this caſe there ſhould be no farther diminution in the bulk of air in proportion as the phoſphorus burns. 3dly. That by reſtoring to a quantity of air, in which phoſphorus has been burnt, and conſequently deprived of its water, and diminiſhed about a fifth, a proportion of water reduced to vapours, an increaſe ſhould be produced in its bulk equal to the diminution which it had ſuffered during the combuſtion. Theſe reflections led me to the following experiments.

EXPERIMENT VI.

To burn Phosphorus under a Receiver immersed in Quicksilver, maintaining an Atmosphere of Water reduced to Vapours under the same Receiver.

I PUT a ſufficient quantity of mercury in a ſmall earthen diſh, and, on its ſurface, placed two ſmall agate cups, in one of which were contained eight grains of phoſphorus, and in the other about a drachm of water; theſe were covered with a cryſtal receiver, and the mercury was raiſed in it to a proper height.

The focus of the burning lens was firſt thrown on the cup which contained the water: in a few minutes it became hot, and preſently boiled and roſe in vapours which condenſed in drops and trickled down the ſides of the receiver. When I was perfectly aſſured that a plentiful atmoſphere of watery vapours exiſted under the receiver, I ceaſed to make the water boil, and threw

threw the focus of the ſame lens on the phoſphorus.

The combuſtion was performed as uſual; the ſame quantity of air was abſorbed, nor did the experiment differ from all thoſe made over quickſilver, except that the acid, inſtead of being in white flowers and in a concrete form, was depoſited in drops on the ſides of the receiver, in proportion to the quantity of water with which it had been ſupplied.

EXPERIMENT VII.

To restore Moisture to Air in which Phosphorus has burnt.

THE ſame experiment was repeated, obſerving to burn the phoſphorus firſt, and afterwards to make the water boil by means of the burning glaſs.

The acid vapours were depoſited on the ſides of the receiver in white flowers, but leſs beautiful than in the fifth experiment; and, in ſome minutes, they deliqueſced, occaſioned by the humidity which the water, though cold, had furniſhed under the receiver. When the veſſels were cooled, the abſorption of air was found to be nearly equal to that of the preceding experiments. I then threw the focus of the lens on the water contained in the cup, and made it boil. The vapour was immediately diffuſed in the area of the receiver, and collected in drops along its ſides; but the height of the quickſilver was neither increaſed nor diminiſhed, ſo that the volume of air remained exactly the ſame.

EXPERIMENT VIII.

To try whether a greater Quantity of Phosphorus can be burnt in a given Quantity of Air, by the Aid of Water reduced to Vapours.

I EMPLOYED, in this experiment, the ſame two agate cups as in the preceding ones; into one

one was put a little diftilled water, and into the other eighteen grains of phofphorus. The water was made to boil by means of the burning glafs, and I then kindled the phofphorus.

No more than feven or eight grains were burnt, after which the combuftion ceafed, and it was not poffible to reanimate it by the help of the burning glafs. The greater part of the unburnt phofphorus remained in the cup; fome portions were fublimed on the inner fides of the receiver. The abforption of air was 18½ inches, i. e. very nearly the fame as in the other experiments.

It appears evident, from thefe experiments, that the diminution in the volume of air obferved during the burning of phofphorus, is not owing to the abforption of water which was contained in it; that the greater or fmaller quantity of water introduced under the receiver, and combined with the enclofed air, makes no alteration in the phenomena; and that the whole difference which refults from it is to have the acid either concrete or fluid. Not that I would deny that the phofphoric acid, when forming, may attract from the air a portion of the moifture with which it is loaded; it is even very probable that this really happens; and it is, doubtlefs, on account of this moifture that the augmentation in weight, obferved in Experiment V. was found to be rather greater than it ought to have been from the quantity of air abforbed; but it does not appear to be the lefs proved by every thing which has preceded, 1ft. That the greater quantity of the fubftance abforbed by phofphorus during its burning, is fomething elfe than water. 2dly. That it is to the addition of this fubftance that the phofphoric acid owes its increafe in weight. 3dly. That to the fubtraction of it, the diminution in the bulk of air in which phofphorus has burned is to be attributed. A concluding experiment which I would introduce by fome preliminary reflections, will I hope carry thefe truths to demonftration.

I SUPPOSE a bottle or fome other veffel with a narrow neck to be exactly filled with diftilled water, in fuch a manner that it would not be poffible to add a fingle drop more without its running

running over the brim. If afterwards ſome phoſphoric acid be introduced into this bottle, or any other acid in a ſtate of abſolute concentration, that is, abſolutely deprived of water, it is clear that one of theſe two things muſt happen; either that the acid muſt be lodged between the particles of water and be combined with it without increaſing its bulk; or rather, which is more probable, that in mixing with the water, it will ſeparate the parts, and a greater volume than that of the water will reſult from the mixture; there would then be a quantity of fluid greater than what the bottle could contain, and this exceſs would trickle over its brim.

Supposing the quantity of acid introduced to be unknown, it will not be difficult to determine it in the firſt caſe; it is only neceſſary to weigh the bottle, and the increaſe of weight which it has acquired will be equal to the weight of the acid which has been added.

The circumſtances will differ in the ſecond caſe; for to have the quantity of acid which has been introduced into the bottle, we ought to add

add to the increafe of weight which it has acquired, the weight of the fluid which may have run over its brim: but it will always be certain, and we may regard it as demonftrated that in both cafes the quantity of acid added, if it be not greater, is at leaft equal to the augmentation in weight which the bottle has acquired. Thefe reflections will naturally apply to the following experiment.

EXPERIMENT IX.

An Examination of the relative Weight of Phosphoric Acid with Distilled Water, and the Consequences deducible from thence.

IN the middle of a large glazed earthen difh, I placed a fmall agate faucer, and covered the whole with a great glafs receiver, but in fuch a manner that the margin of the difh fhould extend farther than that of the receiver. Both veffels had been previoufly moiftened with fome diftilled

diftilled water. The apparatus having been thus difpofed, I put in the agate faucer two or three grains of phofphorus, and inflamed it by means of the blade of a knife moderately heated, which I paffed under the receiver and touched the phofphorus with it. As foon as the inflammation began, a very thick column of white vapours arofe from the phofphorus, and was diffufed in the receiver; but it was remarkable that though the receiver was merely placed on the difh, and did not even touch exactly in all its points, the vapour which circulated within it, inftead of being driven out by the rarefaction occafioned by the heat, feemed on the contrary to be pufhed inwards by the currents of external air which were introduced under the receiver. This circumftance however did not prevent a fmall quantity of vapours from efcaping at fome other moments.

An hour was neceffary to condenfe the whole of the vapours contained under the receiver; after which I recommenced the fame operation, only taking care to moiften the receiver again either with diftilled water, or the fame water which

which had been already employed, and which became more and more acid.

It is proper to obſerve that at the end of each combuſtion, there always remained, in the bottom of the ſaucer, ſome portions of yellow matter, of which I have before ſpoken, and which conſiſt of half decompoſed phoſphorus; I took particular care to lay this aſide. I continued to burn phoſphorus in this manner to the amount of two drachms forty-two grains; when, having waſhed and dried the yellow matter which remained, its weight was thirty-two grains; the quantity of phoſphorus therefore which had been burned, was in reality no more than two drachms ten grains.

The liquor reſulting from this operation was clear and limpid, without colour or ſmell, and had an acid taſte like that of oil of vitriol diluted with a large quantity of water. It was evident that this liquor was only diſtilled water, into which a certain quantity of phoſphoric acid had been introduced; and I was able to apply to it the reflections which preceded this experiment.

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I ACCORDINGLY took a phial capable of containing all the phofphoric acid which I had obtained, and having put the acid into it, as there ftill remained a fmall void fpace between the liquor and the neck of the bottle, I filled it up with diftilled water, and tied a thread exactly level with the furface of the liquor. The bottle having been put into the fcales, the weight of acid, exclufive of the bottle, was 6 ounces, 7 drachms, 69½ grains.

THE bottle was afterwards emptied, and very carefully cleaned and then filled with diftilled water up to the fame mark. The weight of this water, allowance being made for that of the bottle, was found to be 6 ounces, 4 drachms, 42 grains, which makes the excefs of weight in the acid above that of the diftilled water, 3 drachms, 27½ grains.

IT is clear from what has been faid above, that an excefs in weight of 3 drachms, 27½ grains, fhews at leaft that there exifted that portion of acid in the liquor, even upon the moft unfavourable fuppofitions; yet the quantity of phofphorus employed

employed was only 2 drachms, 10 grains; from whence it evidently follows that the phoſphorus had attracted, during its combuſtion, at leaſt 1 drachm, 17 grains of ſome kind of ſubſtance. This ſubſtance could not be water, becauſe *water* could not have augmented the ſpecific gravity of *water*; it was therefore either air itſelf, or ſome other elaſtic fluid contained, in a certain proportion, in the air which we reſpire. This laſt experiment appears ſo demonſtrative to me, that I do not foreſee any objection that can be advanced againſt it.

CHAPTER X.

EXPERIMENTS ON COMBUSTION AND DETONNATION IN VACUO.

IF the combuſtion of phoſphorus conſiſt eſſentially, as the preceding experiments ſhould ſeem to prove, in the abſorption of air, or ſome other

other elaſtic fluid contained in the air, it ſhould therefore reſult that the burning of phoſphorus cannot be performed without air, and conſequently cannot take place in the vacuum of an air pump, and I was curious to procure myſelf this addition of proof.

EXPERIMENT I.

To try to burn Phosphorus in Vacuo.

A LITTLE piece of phoſphorus was placed under the receiver of an air pump, and as perfect a vacuum was made as the machine would admit of. I then threw the focus of a lens of 8 inches diameter on the phoſphorus; it immediately fuſed, bubbled and acquired a colour rather of a deeper yellow than before; at length it ſublimed, but it did not burn at all. Having admitted air into the receiver, and having taſted the watery vapours which adhered internally to its ſides, I did not even find them ſenſibly acid; from whence it is plain there had been no combuſtion.

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EXPERIMENT II.

Sulphur in Vacuo.

SULPHUR expoſed in the vacuum of an air pump to the heat of a burning glaſs, ſublimed like the phoſphorus, nor was it poſſible to ſet it on fire.

EXPERIMENT III.

Gun-Powder in Vacuo.

SOME gun-powder was placed under the receiver of an air pump, and as perfect a vacuum made as could poſſibly be effected. The focus of a burning glaſs being afterwards thrown upon the powder, it fuſed, the ſulphur was ſublimed to the top of the receiver, but it neither took fire nor exploded. I made uſe alſo in this experiment of a lens of eight inches diameter.

HAVING introduced a ſmall quantity of air under the receiver, about a twentieth part of what it was capable of containing, the exploſion was eaſily made, with a noiſe nearly ſimilar to that of the burſting of a thin bladder. The noiſe was proportionably leſs as the receiver was larger.

EXPERIMENT IV.

NITRE AND SULPHUR IN VACUO.

EQUAL parts of nitre and ſulphur do not produce any kind of exploſion in vacuo. The ſulphur ſublimes without burning in the ſame manner as if it were unmixed.

CHAPTER XI.

Of AIR in which PHOSPHORUS has been burnt.

EXPERIMENT I.

The Effect of Air, in which Phosphorus has been burnt, on Animals.

A QUANTITY of air which had been diminiſhed $\frac{1}{11}$ in its bulk by the burning of phoſphorus, was tranſmitted into a jar by means of the pump PP, and by an apparatus nearly ſimilar to that of fig. 10. I threw a bird into it and left it there a full half minute. I did not perceive that its reſpiration was more difficult than in common air, nor did it ſhew any ſigns of being injured. We may recollect on the contrary that an animal, of the ſame ſpecies, being put into fixed air, periſhed almoſt on the firſt inſpiration.

EXPE-

EXPERIMENT II.

THE EFFECT OF AIR, IN WHICH PHOSPHORUS HAS BEEN BURNT, ON LIGHTED CANDLES.

ANOTHER portion of the fame air was thrown into a narrow jar, and a lighted wax candle plunged into it. It was inftantly extinguifhed, as in elaftic fluid from effervefcences and reductions. The candle, having been feveral times lighted anew, it was conftantly extinguifhed; I obferved, however, that this experiment could not be fo frequently repeated in this air, as in that obtained from effervefcing mixtures and reductions; which induces me to believe that it is more eafily and readily mixed with the air of the atmofphere *.

* THIS air was not pure fixed air, but common air in a ftate of decompofition, and from which a fhare of its fixable part had been abforbed by the phofphorus. T. H.

EXPERIMENT III.

To mix a Portion of Elastic Fluid from an Effervescing Mixture, with Air in which Phosphorus has been burnt.

I HAD the curiofity, relative to fome views of which I fhall give an account another time, to obferve whether the mixture of one-third of elaftic fluid from an effervefcing mixture, would correct the air which had been employed for the burning of phofphorus, and reftore to it the property of fupporting flame. A narrow jar was filled with a mixture of them, and a candle introduced into it, but it was immediately extinguifhed.

THE

APPENDIX.

NUMBER I.

A MEMOIR

ON THE NATURE OF THE PRINCIPLE WHICH IS COMBINED WITH METALS DURING THEIR CALCINATION, AND OCCASIONS AN INCREASE IN THEIR WEIGHT *.

Read before the ROYAL ACADEMY, April 26th, by M. LAVOISIER.

DO there exiſt different ſpecies of air? Is it ſufficient that a body be in a ſtate of

* THE firſt experiments relative to this memoir have been made above a year ago; thoſe on the *mercurius precipitatus per ſe*, were firſt tried with a burning glaſs in the month of November 1774, and afterwards made with all the neceſſary precautions and care, in the laboratory of *Montigny*, in conjunction with M. de Trudaine on the 28th. of February, and the 1ſt. and 2d. of March of this year; and were finally repeated again the 31ſt. of laſt March, in preſence of M. the Duke de la Rochefoucault, M. de Trudaine, M. de Montigny, M. Macquer, and M. Cadet.

of permanent expanſibility * to conſtitute a ſpecies of air? And laſtly, are the different kinds of air which nature affords us, or which we are able to form, ſubſtances diſtinct of themſelves, or modifications of atmoſpheric air? Such are the principal queſtions which appertain to the plan which I propoſed to myſelf to lay before the Academy: but the time devoted to our public meetings not permitting me to treat any queſtion in its full extent, I ſhall confine myſelf to day to one particular caſe only, and limit myſelf to ſhew that the principle which is united to metals during their calcination, which increaſes their weight, and which conſtitutes them in the ſtate of a calx, is neither one of the conſtituent parts of the air, nor a particular acid diffuſed in the atmoſphere; that it is the air itſelf undivided, without alteration, without decompoſition, to ſuch a degree, that if after having been engaged in

* This word is at preſent generally received among philoſophers and chemiſts, ſince a modern author has eſtabliſhed the meaning of it, in a very elaborate article replete with views the moſt extenſive and new, and which bear in every part the mark of genius. Vide Encyclopédie, Tome VI. p. 274, at the word *expanſibilité*.

in this combination, it be ſet at liberty, it is ſeparated more pure, more reſpirable, if I may be permitted to uſe the expreſſion, than the air of the atmoſphere, and is more proper to ſupport flame and the combuſtion of bodies.

Most of the metallic calces are not reduced, that is to ſay, do not return to the ſtate of a metal, without the immediate contact of charcoal, or ſome other ſubſtance which contains what is called phlogiſton; the charcoal which is employed is wholly deſtroyed in this operation, when the quantity is well proportioned, and conſequently the air, which is ſeparated in metallic reductions effected by charcoal, is not a ſimple being, but is, in ſome degree, a combination of the elaſtic fluid ſeparated from the metal, and of that diſcharged from the charcoal. Therefore becauſe this fluid is obtained in the ſtate of fixed air, we have no right to conclude that it exiſted in that ſtate, in the metallic calx, previous to its combination with the charcoal.

These reflections made me ſenſible how eſſential it was to the unfolding of the myſtery of the

the reduction of metallic calces, to direct all my experiments to those which are reducible without addition. The calx of iron afforded me this property; indeed, of all those, whether natural or artificial, which we have exposed to the focus of the great burning glass belonging to M. le Regent, or to that of M. de Trudaine, there was not one but what was thereby totally reduced.

In consequence of this, I tried to reduce, by the assistance of a burning glass, several kinds of calces of iron, under large glass receivers inverted into mercury, and I became able to separate from them, by this means, a great quantity of air; but as at the same time this air was found to be mixed with the common air contained within the receiver, this circumstance threw a great uncertainty on my results; no proofs to which I submitted this air were perfectly conclusive, and it was impossible for me to be assured, whether the phenomena which I obtained depended on the common air, on that separated from the calx of iron, or on the combination of the two together. My design not having

having been completed by thefe experiments, I fupprefs the detail of them here; they will, befides, find their proper place in other memoirs.

As thefe difficulties depended on the very nature of iron, on the refractory quality of its calces, on the difficulty of reducing them without addition, I confidered them as infurmountable, and I believed, from this time, that I ought to turn my attention to another kind of calx which would be more eafily managed, and which had, like the calx of iron, the property of being reduced without addition. The *mercurius precipitatus per fe*, which is nothing more than a calx of mercury, as fome authors have already advanced, and as we fhall ftill be more fully convinced by reading this memoir; the *mercurius precipitatus per fe*, I fay, appeared to me proper to fulfil completely the object which I had in view. Indeed it is at prefent known that it is reducible, without addition, in a very moderate degree of heat. Although I have frequently repeated the experiments which I am going to relate, I have not thought it proper to give, at this time, a detail of each of them in particular,

particular, for fear of fwelling this memoir too much, and I have thrown together, in one recital, the circumftances which belong to feveral repetitions of the fame experiment.

To affure myfelf at firft, whether the mercurius precipitatus per fe were a true metallic calx, whether it yielded the fame refults, the fame fpecies of air by reduction, I, in the firft place, tried to reduce it by the common method, or, to ufe the received expreffion, by an addition of phlogifton.

I, ACCORDINGLY, mixed an ounce of this calx with forty-eight grains of powdered charcoal, and introduced the whole into a fmall glafs retort, the utmoft capacity of which did not exceed two cubic inches, and I placed it in a reverberatory furnace proportioned to its fize. The neck of this retort was about a foot in length, and three or four lines in diameter; it had been bent in different places, in an enameller's lamp, and its extremity was fo difpofed, that it could be brought under a glafs receiver, of fufficient dimenfions, filled with water, and inverted

inverted in a ciftern filled with the fame. This apparatus, fimple as it is, is the more exact as it has neither folder nor lute, nor any paffage through which air might be introduced or efcape.

As foon as the fire was placed under the retort, and it received the firft impreffions of heat, the common air which it contained was dilated, and fome little of it paffed into the receiver; but confidering the fmallnefs of the void part of the retort, that air could not make any fenfible error; and its quantity, reckoning it at the higheft, could fcarcely amount to a cubic inch. As foon as the retort began to be much heated, the air was feparated with much rapidity, and mounted, through the water, into the receiver. The operation did not laft above three quarters of an hour, and yet the fire had been kept low during that time. When the whole mercurial calx was reduced, and the air no longer continued to pafs, the height was marked at which the water ftood in the receiver, and the quantity of air feparated was found to be fixty-four cubic inches, without reckoning the portion which muft neceffarily have

have been abſorbed by the water in paſſing through it.

This air was immediately ſubmitted to a great number of trials, the detail of which I am obliged to ſuppreſs, and the reſults were, 1ſt. that it was ſuſceptible of being combined with water by agitation, and of communicating to it all the properties of the acidulous or aërial waters, ſuch as thoſe of Seltzer, Pougues, Buſſang and Pyrmont, &c. 2dly. that animals plunged into it periſhed in a few ſeconds; 3dly. that candles, and all combuſtible bodies in general, were extinguiſhed in it inſtantly; 4thly. that it precipitated lime-water; 5thly. that it very readily combined with alkalis, whether fixed or volatile; that it deprived them of their cauſticity, and gave them the property of chryſtallizing. All theſe qualities are preciſely thoſe of that ſpecies of air, known by the name of fixed or mephitic air, ſuch as is obtained from all metallic calces by the addition of charcoal, ſuch as is ſeparated from effervеſcing and fermenting bodies, and it was therefore confirmed, that *mercurius*

precipitatus

precipitatus per ſe belonged to the claſs of metallic calces.

The only remaining inquiry was to examine this calx alone, to reduce it without addition, to ſee whether it yielded any air by that method; and ſuppoſing that it did yield it, to determine in what eſtate that air might be. For this purpoſe, in a retort, alſo, of two cubic inches in dimenſion, an ounce of *mercurius precipitatus per ſe* was placed, alone, the apparatus was diſpoſed in the ſame manner as in the preceding experiment, and I ſo contrived that all the circumſtances ſhould be exactly ſimilar. The reduction was accompliſhed this time with rather more difficulty than by the addition of charcoal; it required more heat, and the effect was not ſenſible till the retort began to be ſlightly red hot; the air was then ſeparated gradually, and paſſed into the receiver, and, by keeping up the ſame degree of heat for two hours and half, the whole of the mercury was reduced.

The operation being finiſhed there were found, on one part, in the neck of the retort, and

and in a glaſs veſſel which was placed beneath the water under its beak, ſeven ounces eighteen grains of mercury, on the other hand, the quantity of air, which had paſſed into the receiver was found to be ſeventy-eight cubic inches; and therefore, ſuppoſing that the whole loſs of weight ought to be attributed to the air, each cubic inch ſhould weigh ſomething leſs than two thirds of a grain, which does not differ much from the weight of common air.

Having thus fixed the firſt reſults, nothing now remained, but to ſubmit the ſeventy-eight cubic inches of air which I had obtained to all the trials proper for determining its nature, and I diſcovered with much ſurprize, 1ſt. that it was not capable of combination with water by agitation; 2dly. that it did not precipitate lime-water; 3dly. that it did not unite with fixed or volatile alkalis; 4thly. that it did not, at all, diminiſh their cauſtic quality; 5thly. that it would ſerve again for the calcination of metals; 6thly. that it was diminiſhed like common air, by addition of one-third of nitrous air; laſtly, that it had none of the properties of fixed air:

far

far from being fatal, like it, to animals, it seemed, on the contrary, more proper for the purposes of respiration; candles and burning bodies were not only *not* extinguished by it, but burned with an enlarged flame in a very remarkable manner; the light they gave was much greater and clearer than in common air. All these circumstances fully convinced me that this air was not only common air, but that it was even more respirable, more combustible, and consequently more pure even than the air in which we live.

It seems to be proved from hence, that the principle which combines with metals during their calcination, and which occasions the augmentation in their weight, is nothing but an exceedingly pure portion of the air which surrounds us, which we respire, and which passes, in this process, from a state of expansibility to that of solidity: if then it be obtained in the state of fixed air in all the metallic reductions when charcoal is employed, it is to the charcoal that this effect should be attributed; and it is very probable that all the metallic calces would yield only common air if we could reduce them all without addition, in the ſame manner as the *mercurius precipitatus per ſe.*

Every thing which I have ſaid of the air from metallic calces, may naturally be applied to that which is obtained from nitre by detonnation; it is known from ſeveral experiments already publiſhed, moſt of which I have repeated, that the greater part of that air is in the ſtate of fixed air, that it is mortal to animals who breathe it, that it has the property of precipitating lime-water, of uniting alone with lime and alkalis, of rendering them mild, and cauſing them to cryſtallize; but as, at the ſame time, the detonnation of nitre does not take place without the addition of charcoal, or of ſome body which contains phlogiſton, it is very probable that under this circumſtance alſo the common air is converted into fixed air; from whence it ſhould follow that the air combined in nitre, and which produces the terrible exploſions of gun-powder, is common atmoſpherical air deprived of its expanſibility.

As common air is changed into fixed air when combined

combined with charcoal, it ſhould ſeem natural to conclude that fixed air is merely a combination of common air and phlogiſton. This is Dr. Prieſtley's opinion *, and it muſt be granted that it is not improbable; however, when one deſcends to a detail of facts, it is found ſo frequently contradicted, that I think it neceſſary to deſire philoſophers and chemiſts to ſuſpend their judgment; I hope ſoon to be able to publiſh the motives of my doubts.

* Dr. Priestley has, certainly, never delivered ſuch an opinion as M. Lavoiſier here aſcribes to him. His doctrine is, that every diminution of common air is produced by phlogiſton, and that this diminution is owing to the depoſition of one of its conſtituent parts, viz. that principle which is commonly denominated fixed air. But he has never conſidered this fixed air as a compound, but rather as an elementary body. T. H.

NUMBER II.

An ACCOUNT of Dr. PRIESTLEY's OPINION relative to the PRINCIPLE which is combined with METALS during their CALCINATION, and of HIS DISCOVERY of DEPHLOGISTICATED AIR.

IT appears, from the preceding memoir, that M. Lavoiſier not having obtained ſatisfactory reſults from his experiments on the calces of iron, had recourſe to *mercurius calcinatus per ſe*, as being more eaſily reducible without addition. This ſubſtance afforded him not fixed, but, what he imagined to be, common air, in a ſtate rather more pure than that which we uſually breathe; and this circumſtance led him to conclude, perhaps too haſtily, that the air obtainable from all the metallic calces, if it were poſſible to reduce them without addition, would be of the ſame kind,

kind, and that the fixed air which is produced from reductions in the common method, does not proceed from the calx but from the charcoal employed in the process.

Dr. Priestley, however, differs in opinion from his ingenious fellow labourer on this point. He declares that several of the metallic calces yielded fixed air, by heat only, without any addition of charcoal, when exposed to the action of a large burning lens of twelve inches diameter and twenty inches focal distance.

This excellent philosopher also, from whom M. Lavoisier seems to have received the first idea* of extracting a particular kind of air from *mercurius calcinatus*, has extended his researches much farther into the nature of the air obtained from that calx. These inquiries have led him to discoveries and reflections, relative to the nature and composition of atmospheric air, of the highest importance to physics, and which may, perhaps, in time, prove productive of consequences the most beneficial to mankind. Having obtained

* See Priestley's Experiments and Observations, Vol, II. page 36, and 320.

obtained air, from *mercurius calcinatus*, which was not abſorbed by water, and in which a candle burned with an enlarged flame, he, at firſt, imagined it to be nothing more than air in the ſame ſtate to which he had formerly brought nitrous air by expoſing it to iron filings or liver of ſulphur, and he little ſuſpected that it was of that ſuperior degree of purity which he has ſince proved it to poſſeſs. He afterwards procured air of a ſimilar kind from common *red precipitate*, and ſometimes, but not uniformly, even from red lead.

By a gradual train of experiments, Dr. Prieſtley, after having remained for ſeveral months without ſuſpicion of the real properties of this air, diſcovered that not only candles burned in it with a more vigorous flame, but that animals lived in it, at leaſt, three times as long as in common air, and that when they were withdrawn from the jar, the remaining air ſtill continued purer than that of the atmoſphere. On trying it by the teſt of nitrous air, he found it to be between four and five times as good as the air we breathe, viz. that whereas one meaſure of of nitrous air is ſufficient to produce the utmoſt poſſible diminution in two meaſures of common air, an equal quantity of this purer air required more than four meaſures to effect the ſame diminution. In the courſe of his experiments, he arrived at the power of procuring air even purer than this, and between five and ſix times as good as common air. This property of abſorbing ſo large a proportion of nitrous air, he judged to depend on its being capable of taking more phlogiſton from that body, as originally containing leſs of this principle, and he conſequently denominated it *dephlogiſticated air*.

The circumſtance of the *red precipitate*, or *mercurius corroſivus ruber*, which is produced from a ſolution of mercury in nitrous acid, yielding dephlogiſticated air, induced Dr. Prieſtley to conclude that the properties of this air might depend on ſomething being communicated to it by the nitrous acid, and that the *mercurius calcinatus* had, in the degree of heat to which it is expoſed in its preparation, attracted ſomething nitrous from the atmoſphere. He was ſoon confirmed in this opinion, for endeavouring to extract air from ſome red lead which had been recently prepared and afforded it but in ſmall quantity, he was deſirous of bringing it to that ſtate in which other red lead had yielded it plentifully; and concluding that the calx muſt imbibe ſome kind of acid from the air, he moiſtened three ſeparate half ounces of the freſh prepared minium with each of the three mineral acids, and having dried, he afterwards expoſed them to a ſufficient degree of heat in a gun-barrel to which a ſuitable apparatus was affixed. From the parcels moiſtened with the vitriolic and marine acids no air was produced, but from that to which the nitrous acid had been added, much air was obtained, the greateſt part of which was fixed air, though there was alſo a conſiderable portion of the dephlogiſticated ſpecies. In this experiment the minium had been repeatedly moiſtened with nitrous acid and dried again, but when, afterwards, it was only once moiſtened with that acid, though leſs air was obtained, it proved to be almoſt all of the dephlogiſticated kind, and was about five times as pure as common air.

HE

He then proceeded to try different ſubſtances, and procured dephlogiſticated air from every kind of earth, which is void of phlogiſton, made into a paſte, as above, with ſpirit of nitre. The metallic earths, in this ſtate, and chalk, appeared to be the beſt adapted to this purpoſe. When theſe ſubſtances had yielded all the dephlogiſticated air that could be extracted from them, they would again afford as much as at firſt, on being moiſtened with freſh ſpirit of nitre, and the proceſs might be repeated till all the earthy matter be exhauſted.

From hence Dr. Prieſtley was led to conclude "that atmoſpherical air, or the thing that we breathe, conſiſts of the nitrous acid and earth, with ſo much phlogiſton as is neceſſary to its elaſticity; and likewiſe ſo much more as is required to bring it from its ſtate of perfect purity to the mean condition in which we find it."

Dephlogisticated air does not only ſupport the lives of animals confined in it, and the flame of burning bodies more powerfully than common air, but alſo conveys ſounds with greater force. An

An explofion of two thirds of inflammable air mixed with rather more than one-third of dephlogifticated air, feemed to be forty times louder than when made with two thirds of atmofpheric air.

DR. PRIESTLEY has, with great reafon, formed very high expectations of the falutary and ufeful purpofes to which this pure air may be applied, and feems to think that, in time, it may even become a fafhionable article of luxury. "Hitherto," fays he, "only two mice and myfelf have had the privilege of breathing it."

BY thefe extracts which Doctor Prieftley (with that friendly difpofition and that defire of contributing to the improvement of fcience, which fo remarkably diftinguifh him) has permitted me to make from his recent and very valuable publication, it appears that he thinks, contrary to the opinion of M. Lavoifier, as advanced in the laft article, that the principle imbibed by metals during their calcination, and which conftitutes the increafe in their weight, is, in fome inftances, fixed air, and in others nitrous

acid,

acid, attracted by them from the common air in which they are calcined.

It alſo appears that this philoſopher, inſtead of regarding atmoſpheric or common air (when free from the foreign matters which are always ſuppoſed to be diſſolved, and intermixed with it) as a *ſimple elementary ſubſtance*, conſiders it as a mixture of ſeveral principles capable of decompoſition; and that he has diſcovered a method of compounding common reſpirable air, by adding to dephlogiſticated air ſuch a quantity of phlogiſton as reduces it to the ſtate in which we generally find the atmoſphere ſurrounding the planet which we inhabit.

It has been ſeen, page 409, that M. Lavoiſier having embraced the hypotheſis of common air being converted into fixed air by combining with phlogiſton, imagined it probable that in the detonnation of nitre or of gun-powder, a converſion of the common into fixed air might reſult from the charcoal which is neceſſary to produce the exploſion. From nitre, heated in a glaſs veſſel, Dr. Prieſtley procured very pure dephlo-

dephlogiſticated air. A mixture of equal parts of brimſtone and nitre yielded air which was highly nitrous, and as the produce of nitre and charcoal is nitrous air, he concludes that this kind of air is alſo produced in the exploſion of gun-powder.

PL. I.

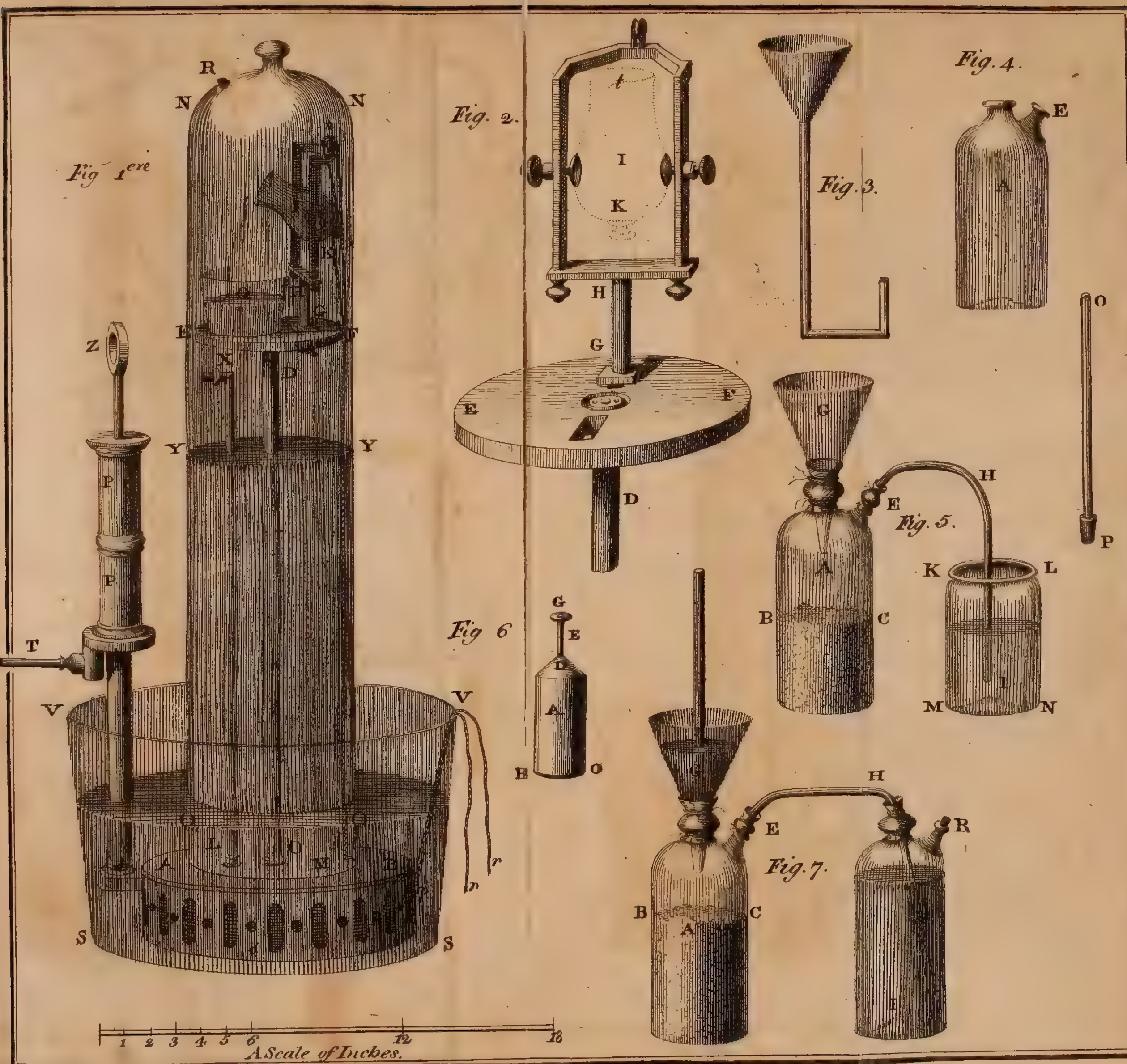
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Fig. 5.
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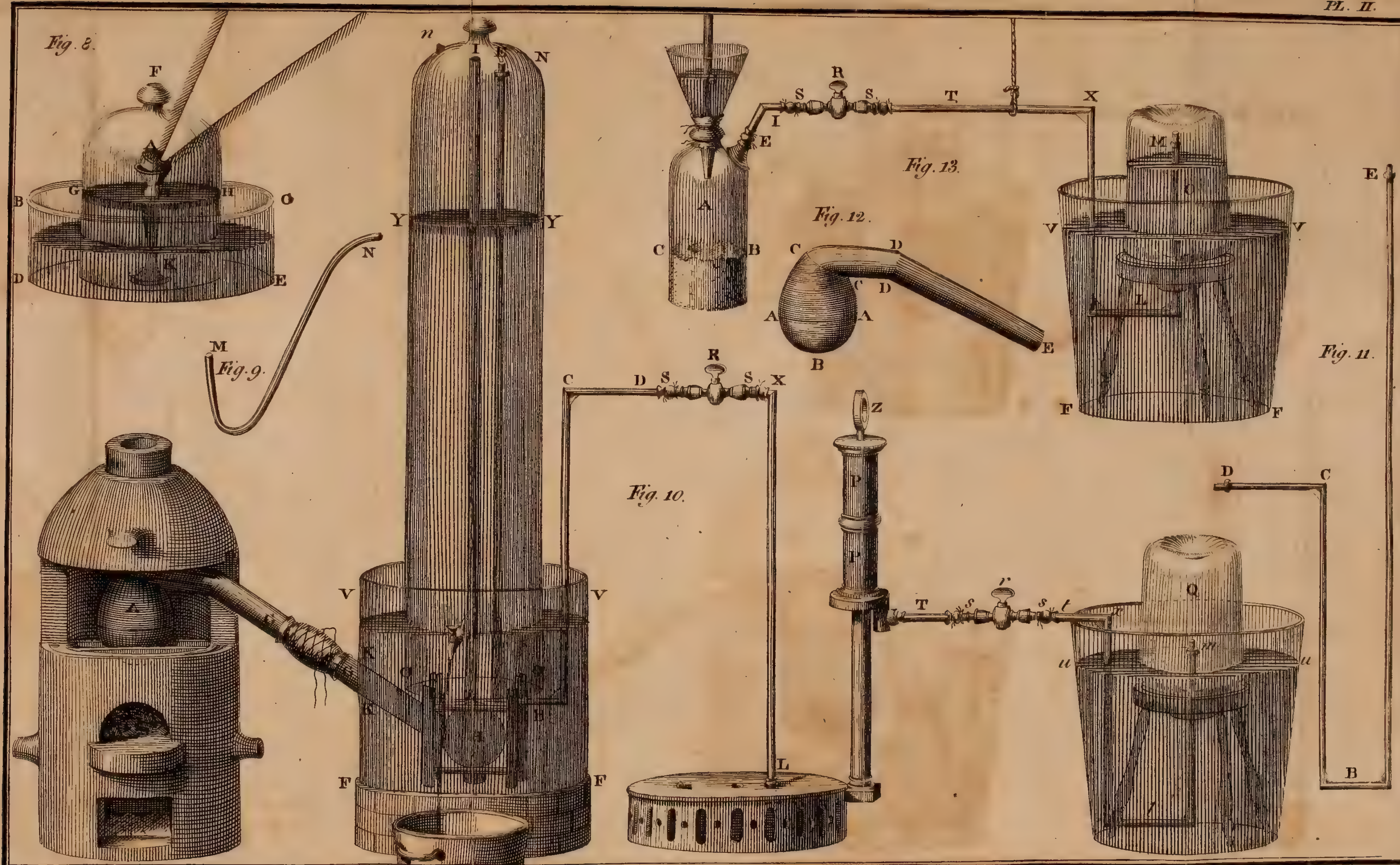
Fig. 7.
1 2 3 4 5 6 12 18
A Scale of Inches.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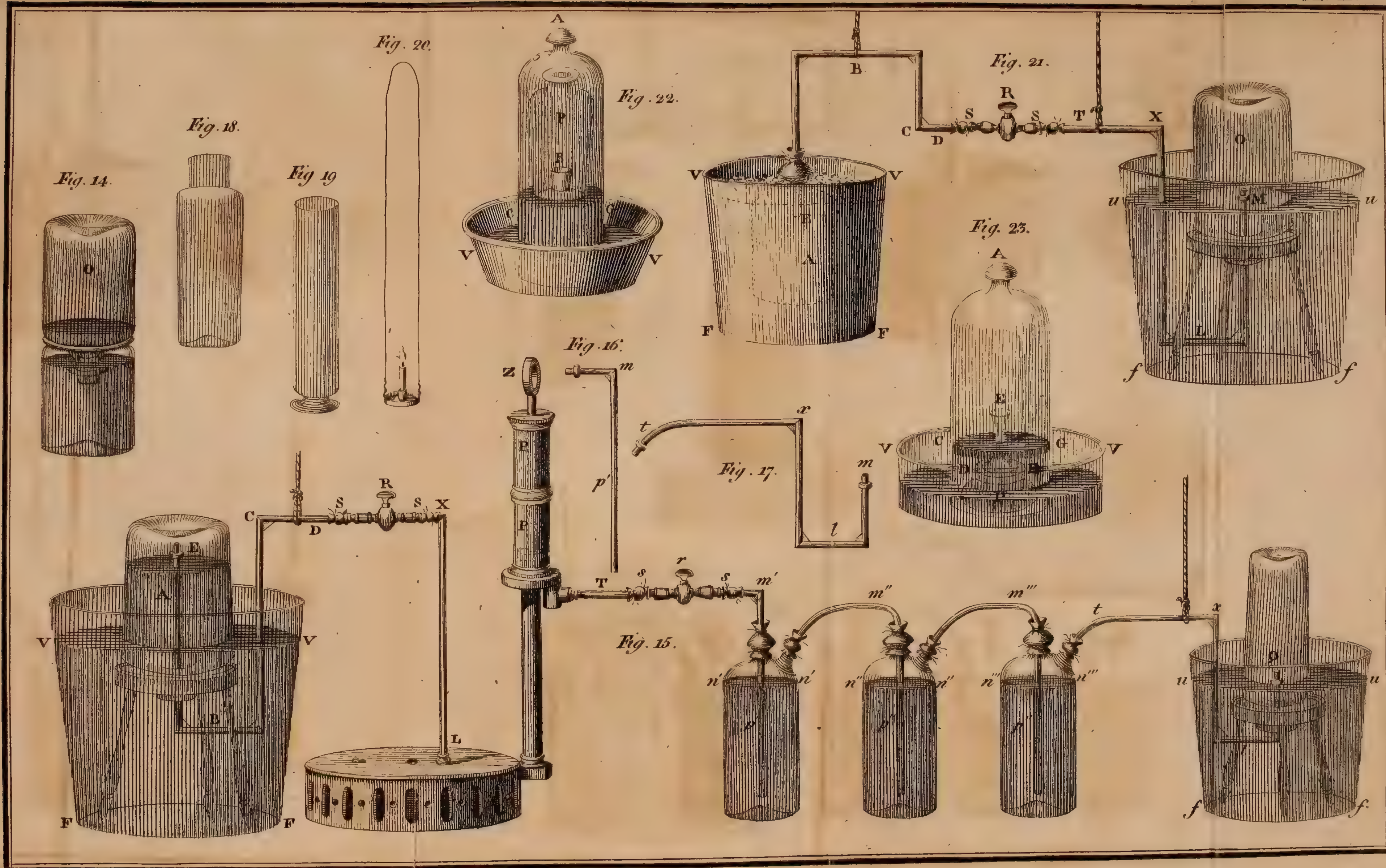
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Fig. 22.
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Fig. 16.
Fig. 17.
Fig. 15.

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